THE QUARTERLY

JOURNAL OF SCIENCE,

AND ANNALS OF

MINING, METALLURGY, ENGINEERING, INDUSTRIAL ARTS, MANUFACTURES, AND TECHNOLOGY.

EDITED BY

WILLIAM CROOKES, F.R.S., &c.

No. LXI.

JANUARY 1879.

LONDON:

OFFICES OF THE QUARTERLY JOURNAL OF SCIENCE,
3, HORSE-SHOE COURT, LUDGATE HILL.

Where Communications for the Editor and Books for Review may be addressed.

Paris:
FRIEDRICH KLINCKSIECK.

Leipzig:
ALFONS DÜRR.

Berlin:
A. ASHER and CO., 53, MOHRENSTRASSE.

PRICE FIVE SHILLINGS.

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I. ON THE THICKNESS OF THE ANTARCTIC ICE, AND ITS RELATIONS TO THAT OF THE GLACIAL EPOCH.

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IN a recent work* I have endeavoured to show that the thickness of the ice on the Antarctic continent must be far greater than is generally supposed; that whatever be its depth at the edge of the continent, where it breaks up into bergs, the thickness at the Pole or centre of dispersion must be enormous.

Since the publication of my work, however, Sir Wyville Thomson, Director of the Scientific Staff of the *Challenger* Expedition, in a lecture on the condition of the Antarctic regions, delivered at Glasgow, has come to a totally different conclusion. His conclusion is based chiefly on considerations relating to the principle of regelation and the physical nature of ice; and the following quotation from his lecture will show his views on the subject:—

"There is one point in connection with the structure of icebergs which is of great interest, but with regard to which I do not feel in a position to form a definite judgment. It lies, however, especially within the province of a distinguished professor in the University of Glasgow, Dr. James Thomson, and I hope he will find leisure to bring that knowledge to bear upon it which has already thrown so much light upon some of the more obscure phenomena of ice. I have mentioned the gradual diminution in thickness

* Climate and Time, Chap. XXIII.

of the strata of ice in a berg from the top of the berg downwards. The regularity of this diminution leaves it almost without a doubt that the layers observed are in the same category, and that therefore the diminution is due to subsequent pressure or other action upon a series of beds which were at the time of their deposition pretty nearly equally thick. About 60 or 80 feet from the top of an iceberg the strata of ice, a foot or so in thickness, although of a white colour, and thus indicating that they contain a quantity of air and that the particles of ice are not in close apposition, are still very hard, and the specific gravity of the ice is not very much lower than that of layers not more than 3 inches thick nearer the water-line of the berg. Now it seems to me that this reduction cannot be due to compression alone, and that a portion of the substance of these lower layers must have been removed.

"It is not easy to see why the temperature of the earth's crust, under a widely extended and practically permanent ice-sheet of great thickness, should ever fall below the freezing-point, and it is a matter of observation that at all seasons of the year vast rivers of muddy water flow into the frozen sea, from beneath the great glaciers which are the issues of the ice-sheet of Greenland. Ice is a very bad conductor, so that the cold of winter cannot penetrate to any great depth into the mass. The normal temperature of the crust of the earth at any point where it is uninfluenced by cyclical changes is, at all events, above the freezing-point, so that the temperature of the floor of the ice-sheet would certainly have no tendency to fall below that of the stream which was passing over it. The pressure upon the deeper beds of the ice must be enormous; at the bottom of an ice-sheet 1400 feet in thickness it cannot be much less than a quarter of a ton on the square inch. It seems therefore probable that, under the pressure to which the body of ice is subjected, a constant system of melting and regelation may be taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and finally working out channels for itself between the ice and the land, whether the latter be sub-aërial or submerged.

"I should think it probable that this process, or some modification of it, may be the provision by which the indefinite accumulation of ice over the vast nearly level regions of the 'Antarctic Continent' is prevented, and the uniformity in the thickness of the ice-sheet is maintained; that, in fact, ice at the temperature at which it is in contact

with the surface of the earth's crust within the Antarctic regions, cannot support a column of itself more than 1400 feet high without melting."*

The subject is one of very considerable importance, not merely in relation to the Antarctic regions at the present day, but also in its bearings on the condition of things generally during the Glacial Epoch. For if Sir Wyville Thomson's conclusions in reference to the thickness of the Antarctic ice be true, they must hold equally true for the ice of the Glacial Epoch, and consequently would modify to a large extent prevailing conceptions regarding the physical condition of our country during that epoch.

They are therefore conclusions worthy of discussion, and, as they are diametrically opposed to those arrived at by myself, I have thought of considering the subject in somewhat fuller detail, the more so as new elements in the question have since been introduced.

At the very outset of the inquiry it must be observed that the question of the thickness of the ice covering the Antarctic continent is one which cannot be determined by direct observation. No one, as yet, has ever been able to set his foot on that continent; and the perpendicular wall forming the outer edge of its icy mantle is nearly all that has been seen of it. Direct measurements, and some other facts to which we shall shortly refer, show with tolerable certainty what is the probable average thickness of the ice-sheet at its outer circumference; but observation can tell us nothing whatever about the thickness of the ice in the interior, which is the question at issue. This has to be determined by purely physical and mechanical considerations, based, it is true, on data derived from observation. A visit to the Antarctic regions may indeed enable us to become acquainted with data which we might not have known otherwise, but this acquaintance would not aid us in drawing the proper inference from those data.

It fortunately happens, however, that the very circumstances that render the region so difficult to get at are those which at the same time tend to simplify the problem. The Antarctic region is the most inaccessible on the globe, but of all regions it is the one where the physical conditions are most uniform and least under the influence of contingent circumstances, such as those resulting from the presence of warm ocean currents in one place and cold currents in another, or of great masses of land in one part

* Condition of the Antarctic Region, p. 23. Nature, vol. xv., p. 122.

and an open sea in another. We have not in the Antarctic, as in the Arctic region, well-marked warm and moist aërial currents and cold and dry winds blowing athwart different areas. Surrounding the South Polar continent lies an unbroken ocean, in an almost uniform climatic condition. This region also, as Sir Wyville Thomson remarks, is "continuously solid,—that is to say, it is either continuous land or dismembered land fused into the continental form by a continuous ice-sheet." In this case we can treat it as one continuous continent. The South Pole being safely assumed to be in the centre of the sheet, we have here what we perhaps never had on the northern hemisphere even during the Glacial Epoch—a polar *ice-cap*. We have the pole in the centre of the cap; therefore, at equal distances from the pole or centre, the conditions in every respect, both as to climate and the thickness of the ice, may be assumed to be the same; for no reason can be assigned for supposing the conditions in separate areas upon the same parallel of latitude to differ. Thus, as a purely physical and mechanical problem, the conditions could hardly be more simplified.

We shall now enter into the consideration of the question. In the first place, the conclusion that "ice at the temperature at which it is in contact with the surface of the earth's crust within the Antarctic regions cannot support a column of itself more than 1400 feet high without melting" is in direct opposition to known facts.

The immense tabular icebergs found in the Southern Ocean, which have been so well described by Sir Wyville Thomson, are of course portions broken off the edge of the ice-sheet, and the thickness of the bergs represents the thickness of the ice-sheet at the place where they broke off. Now, some of these icebergs have been found to be more than three times the limit assigned by Sir Wyville. The following are a few out of the many examples which might be adduced of enormous icebergs, taken from the Twelfth Number of the "Meteorological Papers" published by the Board of Trade, and from the excellent paper of Mr. Towson on the "Icebergs of the Southern Ocean," published also by the Board of Trade.

Sept. 10th, 1856.—The *Lightning*, when in lat 55° 33′ S., long. 140° W., met with an iceberg 420 feet high.

Nov., 1839.—In lat. 41° S., long. 87° 30′ E., numerous icebergs 400 feet high were met with.

Sept., 1840.—In lat. 37° S., long. 15° E., an iceberg 1000 feet long and 400 feet high was met with.

Feb., 1860.—Captain Clark, of the *Lightning*, when in lat. 55° 20′ S., long. 122° 45′ W., found an iceberg 500 feet high and 3 miles long.

Dec. 1st, 1859.—An iceberg, 580 feet high and from 2½ to 3 miles long, was seen by Captain Smithers, of the *Edmond*, in lat. 50° 52′ S., long. 43° 58′ W. So strongly did this iceberg resemble land that Captain Smithers believed it to be an island, and reported it as such, but there is little or no doubt that it was in reality an iceberg. There were pieces of drift-ice under its lee.

Nov., 1856.—Three large icebergs, 500 feet high, were found in lat. 41° S., long. 42° E.

Jan., 1861.—Five icebergs, one 500 feet high, were met with in lat. 55° 46′ S., long. 155° 56′ W.

Jan., 1861.—In lat. 56° 10′ S., long. 160° W., an iceberg 500 feet high and half a mile long was found.

Jan., 1867.—The barque *Scout*, from the West Coast of America, on her way to Liverpool, passed some icebergs 600 feet in height and of great length.

April, 1864.—The *Royal Standard* came in collision with an iceberg 600 feet in height.

Dec., 1856.—Four large icebergs, one of them 700 feet high and another 500 feet, were met with in lat. 50° 14′ S., long. 42° 54′ E.

Dec. 25th, 1861.—The *Queen of Nations* fell in with an iceberg in lat. 53° 45′ S., long. 170° W., 720 feet high.

Dec., 1856.—Capt. P. Wakem, ship *Ellen Radford*, found, in lat. 52° 31′ S., long. 43° 43′ W., two large icebergs, one at least 800 feet high.

Mr. Towson states that one of our most celebrated and talented naval surveyors informed him that he had seen icebergs in the southern regions 800 feet high.

March 23rd, 1855.—The *Agneta* passed an iceberg in lat. 53° 14′ S., long. 14° 41′ E., 960 feet in height.

Aug. 16th, 1840.—The Dutch ship *General Baron Von Geen* passed an iceberg 1000 feet high in lat. 37° 32′ S., long. 14° 10′ E.

From the fact that the ice forming the upper layers of the iceberg is less dense than that of ordinary ice, Sir Wyville Thomson estimates that as much as one-seventh part of the berg may be above water-line. But for the following reasons I am unable to agree with this estimate. It is true, as he remarks, that the white ice which forms the upper portion

of the berg is less dense than ordinary ice, being composed of recent snows; but, on the other hand, this will be counterbalanced by the greater density of the lower portions of the berg which have been subjected for ages to enormous pressure. I hardly think that there is any good reason to conclude that the *mean* density of the bergs is much under that of ordinary ice, namely, 0·92.*

But even if we admit that as much as one-seventh of the berg is above water, still a berg 500 feet in height would be 3500 feet in thickness, and one 600 feet would be 4200 feet thick, while one 720 feet high, of the tabular form, would be 5040 feet, or nearly a mile in thickness.

It would not, of course, be safe to conclude that the thickness of the ice below water bears always the same proportion to the height above. If the berg, for example, be much broader at its base than at its top, the thickness of the ice below water would bear a less proportion than that indicated by the difference of specific gravity of ice and water. But a berg such as that recorded by Captain Clark, 500 feet high and 3 miles long, may be relied upon as having the proportionate thickness under water. The same may be said of the one seen by Captain Smithers, which was 580 feet high, and so large that it was taken for an island.

It may be here remarked that a berg does not stand higher out of the water because the lightest side happens to be uppermost. The height above water is determined by the *mean* density of the berg, and is the same no matter how the various densities may be distributed through the mass. It would be the same though the berg were turned upside down. This follows as a necessary consequence from the fact that the amount of water displaced by the berg is equal to its weight, and of course it is the same whatever side be uppermost.

To evade the force of the evidence derived from the testimony of the icebergs, it is asserted by some that the heights thus recorded are mere guesses, and not the result of actual measurement. But such an opinion is in direct contradiction to the express declaration of Admiral FitzRoy, who

* It is true that, from observations made (Quart. Journ. Geol. Soc., Feb., 1877) on the density of ice in Disco Bay, Mr. Amund Helland found that, in consequence of the amount of air-bubbles contained in the ice, its density was only 0·886, and from this he concluded that one-seventh of the bergs was above water. But he does not state at what part of the berg his specimens were taken. If they were taken from near the top, or even at the water-line, it might have been expected that the density would be very considerably under that of ordinary ice.

collected the evidence on the subject. He states that "by angular and reliable measurements some of them have been found to be six or eight hundred feet high and several miles in circumference."

But more than this, if Capt. Smithers, for example, did not actually measure the iceberg to which we have referred, he could not have known that its height was 580 rather than 600 feet. The very fact that he stated it to be 580, and not 500 or 600, or even 550 feet, surely implies that he really measured it. The assertion that a person is, for example, 5 feet 6 inches or 5 feet 8 inches in height would not imply that this was his measured height; but if we asserted that he was 5 feet 6½ inches or 5 feet 8¾ inches high, we should necessarily convey this impression. In like manner Captain Smithers, by assigning to the iceberg an altitude so particular as that of 580 feet, distinctly conveys the impression that such was the height obtained by actual measurement. Similarly we conclude that the captains of the *Queen of Nations* and the *Agneta* actually measured the icebergs which they respectively declared to be 720 and 960 feet high.

In reply it may perhaps be asserted that no record of an iceberg 500 or 600 feet in height is to be found in the log-books of the Navy, and that all those instances of enormous icebergs have been given by masters of merchant vessels, who as a rule are not so competent to make accurate measurements. It is doubtless true that the latter are generally not so well qualified for such work as naval officers; but it is hardly credible that they should all have gone so far astray in their measurements as to estimate heights at 500 and 600 feet which in reality were only 200 feet. Now, if but one berg 500 feet high has ever been seen in the Southern Ocean, it is proved that even twice 1400 feet is not the limit of the thickness of the Antarctic ice.

But it is not true that no naval officer has met an iceberg of those enormous dimensions; for, as we have already seen, Mr. Towson states that one of our most celebrated and talented naval surveyors informed him that he had met icebergs in the southern regions 800 feet high. It is, however, not to be wondered at that so few naval officers have seen such bergs, for they are of very rare occurrence. They have been met with chiefly in latitudes that are traverssd by thousands of merchant ships for one vessel belonging to the Navy. And perhaps not one out of every ten thousand merchantmen has ever fallen in with one of the great ice-islands we now speak of.

The testimony from icebergs may therefore be regarded as decisive against the opinion that the Antarctic ice cannot be more than 1400 feet thick.

That 1000 or 2000 feet cannot be the limits of thickness attained by continental ice is amply proved by the geological evidence, which goes to show that during the Glacial Epoch the ice in some places much exceeded 1400 feet. Prof. Dana, for example, has proved that during the period in question the thickness of the ice on the American continent must in many places have been considerably above a mile. He has shown that over the northern border of New England the ice had a mean thickness of 6500 feet, while its mean thickness over the Canada water-shed, between St. Lawrence and Hudson's Bay, was not less than 12,000 feet, or upwards of 2¼ miles (see "American Journal of Science and Arts" for March, 1873). Prof. Erdmann and Mr. Amund Helland have shown that the ice in some parts of Scandinavia was at least 6000 feet thick. It has been proved, by M. Guyot and others, that the Great Valley of Switzerland was formerly filled with a mass of ice between 2000 and 3000 feet in thickness. Mr. Jamieson found that the isolated mountain of Schiehallion, in Perthshire, 3500 feet high, is marked near its top as well as on its flanks, and this not by ice flowing down the side of the hill itself, but by ice passing over it from the north. Dr. James Geikie has shown* that the ice between the mainland and the Outer Hebrides was as much as 3700 feet in thickness. The great mass of ice from Scandinavia, filling the Baltic and the North Sea during the Glacial Epoch, must have been over 3000 feet thick at least.†

The Temperature of the Antarctic Ice.

In examining the physical reasons which have been advanced for the limit assigned to the thickness of the Antarctic ice-cap, we must first consider the probable temperature of the ice; for not only does the thickness of the sheet depend, as we shall see, to a considerable extent on the temperature of the ice, but misapprehensions on this point will tend to vitiate all our reasoning on the subject.

There are but three quarters from which the ice-cap can receive an appreciable amount of heat, viz., (1) the air above; (2) the earth beneath; and (3) the work of compression. Other sources can yield little, if any at all. For

* Quart. Journ. Geol. Soc., vol. xxxiv., p. 861.
† Climate and Time, Chap. XXVII.

instance, the amount carried inward horizontally from the outer edge of the cap by conduction must be infinitesimal, and indeed can never affect the interior, as the ice moves outward more rapidly than the heat can possibly travel inward.

Heat derived from Beneath.—We shall begin with the consideration of the heat derived by the bottom of the sheet from the earth's crust. The researches of Sir William Thomson enable us to determine with a tolerable degree of certainty the amount received from this source. He tells us that through every square metre of the earth's surface 220 metre-tons, or 1,613,700 foot-pounds, of underground heat pass upwards annually. Through every square foot, therefore, there must come 149,600 foot-pounds. This amount is sufficient to melt a layer of ice, already at the melting-point, one-fifth of an inch in thickness. But underground heat would probably be insufficient to melt even so small a layer, since a portion of the heat must, doubtless, be expended in passing through and maintaining at the melting-point a few inches of the ice at the bottom of the sheet.

At first we might be apt to suppose that underground heat ought to travel up through the ice in the same way as through the strata of the earth below, and to make its presence sensibly felt at no great distance from the surface of the sheet. This, however, is impossible; for (1) the greater part of the heat is spent not in raising the temperature, but in melting the ice; and (2) the ice when melted immediately runs off, carrying the heat along with it. But it will be replied, that, notwithstanding this, if the temperature of the ice be much below the freezing-point, the heat constantly passing into the solid layers at the bottom of the sheet, though trifling, ought in course of ages to pass up through the ice, affecting its temperature not for a few inches, as I have supposed, but for a thickness of a great number of feet. Were the ice, like the ground underneath on which it rests, to remain immovable, this would no doubt be the case; but the sheet is in a state of constant motion outwards from the centre of dispersion, and no sooner is a particle of the ice heated than it moves away, and its place is supplied by another particle from behind, which in turn requires to be heated. Besides, the ice has always a downward as well as a horizontal motion; for all the ice found at the bottom comes primarily from the top, and that removed from below is replaced from above. Hence not only

is internal heat from below carried away by the horizontal flow of the ice, but the upward motion of the heat is checked by a downward flow of the ice from above; and the ice is, in all probability, moving downwards more rapidly than the heat is travelling upwards. We must therefore conclude that underground heat is confined to a very thin layer of the cap at the bottom, and that its effects, either in melting the ice or in raising its temperature, are so trifling that they may be practically disregarded in our present inquiry.

It must further be observed that when it is stated that underground heat will maintain at the melting-point the ice in contact with the ground, it is not meant that it will maintain it at the temperature of 32°, for, as Prof. James Thomson discovered, the temperature at which the ice melts is lowered by pressure at the rate of about 0·0137° F. for every atmosphere of pressure. In the present case the pressure depends upon the thickness of the ice: so that, if the sheet be 1400 feet deep, the melting-point will be 31·5°; if half a mile deep, it will be 31°; if 1 mile deep, 30°; and so on.*

Heat derived through the Upper Surface.—It follows, from what has already been shown, that the greater part, if not nearly all, the heat possessed by the ice must have been received through the upper, and not the under, surface of the sheet. But what we are at present concerned with is not so much the amount of heat received by the ice as the temperature at which the heat can maintain the ice.

* The melting-point does not, however, vary uniformly with the pressure; for Mousson (Ann Chim. et Phys., 3rd series, lvi., p. 257, 1859) found that it required a pressure of 13,000 atmospheres to lower the melting-point to zero, F., whereas if the melting-point had decreased in proportion to the increase of pressure, a pressure of 2337 atmospheres would have been sufficient. Boussingault succeeded in lowering the melting-point 11° below zero, F., but the amount of pressure employed was not determined (Ann. Chim. et Phys., xxvi., p. 544, 1872).

The fact that the melting-point of ice would be lowered by pressure, or rather that pressure would prevent freezing, was suggested nearly a century ago by Dr. Charles Hutton, Professor of Mathematics in the Military Academy of Woolwich. From certain experiments on the expansive force of ice, made in Canada by Major Williams, in the year 1784-85, Dr. Hutton makes the following remarks:

"From these ingenious experiments we may draw several conclusions:—First. We hence observe the amazing force of the expansion of the ice, or the water in the act of freezing; which is sufficient to overcome perhaps any resistance whatever; and the consequence seems to be, either that the water will freeze, and, by expanding, burst the containing body, be it ever so thick and strong; or else, if the resistance of the body exceeds the expansive force of the ice, or of water in the act of freezing, then, by preventing the expansion, *it will prevent the freezing, and the water will remain fluid, whatever the degree of cold may be.*" (Trans. Roy. Soc. Edin., vol. ii., p. 27.)

In short, the question to be determined is—What is the temperature of the Antarctic ice? Now, if nearly all the heat possessed by the ice has been received from the upper surface of the sheet, the temperature of the mass must be mainly determined by that of the surface, and cannot be far above the mean temperature of the surface. If so, the temperature of the ice must evidently be very considerably below the freezing-point.

(1.) If we suppose the heat to be transmitted from the surface downwards by *Conduction*, we must necessarily conclude that the surface is at a higher temperature than the ice below; for conduction can only take place from a hot to a colder body, and this process could not possibly maintain the mass of the ice below at a temperature equal to the mean temperature of the surface. The general tendency of conduction would, therefore, be to keep the ice beneath at a lower temperature than that at the surface.

(2.) The work of *Radiation*, however, would probably have the opposite tendency. The heat received by direct radiation from the sun could not possibly raise the temperature of the ice above 32°, but the heat lost by radiation might lower the temperature to far more than 32° below zero. If the heat received from the sun's rays should keep the surface of the ice at, say, 32° during the summer, and the heat lost by radiation should keep the surface at −32° during winter, which is not an extravagant supposition, the mean temperature of the surface would then be 0°. But the mean temperature of the underlying ice would not be so low; for the low mean temperature of the surface is almost wholly due to loss by radiation into stellar space during winter, and this loss would be chiefly confined to the surface. Had the surface been rock instead of ice the rise of temperature during summer would have been about as great as the decrease during winter, and consequently the mean temperature would have been 32° instead of 0° as in the case of ice. Hence the difference between the mean temperature of a rock surface and that of the rock below would not be so great as in the case of ice. The tendency of direct radiation, therefore, is to maintain the surface of the ice-sheet at a lower temperature than that of the underlying mass.

(3.) This tendency is strengthened by another circumstance which comes into operation. During summer a large portion of the direct heat from the sun is spent in melting the surface ice. The melted ice passes down through crevasses and openings in the sheet, thus carrying the temperature along with it. The heat of summer is by

this means carried down below the surface, but not so the cold of winter.

The melting of the ice on the Antarctic continent will be greatly retarded, however, by the coldness of the air, the temperature of which, even during summer, is considerably below the freezing-point. A wind, a few degrees below the freezing-point, blowing on the icy surface would probably re-freeze the ice as rapidly as the sun's rays could melt it. These conditions differ entirely from those that obtain in the Arctic regions. In the latter the air in summer is above the freezing-point, and consequently assists the sun in melting the ice, whereas in the Antarctic regions it is below the freezing-point, and tends to prevent the sun from melting the ice. This circumstance explains the fact, which so much surprised Sir James Ross, that no streams of water flow off the Antarctic ice, similar to those that escape from the great ice-fields of Greenland.

Such water as formed on the surface could not penetrate to any considerable depth, for the ice, as we shall presently see, being much below the freezing-point, the water would be re-frozen before it could descend to any great depth.

It therefore follows that the great mass of ice, up to within a short distance of the surface, can be very little affected by heat transmitted either by conduction, or by radiation, or by water from ice melted at the surface.

Heat derived from Work of Compression and Friction.—I shall now consider the third and last source from which the ice can obtain heat, viz., *Work of Compression and Friction*. We are fortunately able to come to a pretty definite conclusion in regard to the total amount of heat derivable from this source. The force employed is Gravity, and we can thus determine with certainty the greatest amount of work which that can possibly perform on the ice. Mere pressure, however great, cannot of course generate heat unless it perform work, and the heat thus generated is not proportionate to the pressure, but to the work performed, and the amount of work done by pressure is proportionate to the space through which the pressure continues to act. When the pressure is gravity, the work is measured by the distance that the body is allowed to descend. A pound weight descending 1 foot performs 1 foot-pound of work; descending 2 feet it performs 2 foot-pounds; and so on in proportion to the number of feet of descent. In estimating the total amount of work which gravity can perform in the descent of a glacier down the side of a mountain, we measure the work by the vertical distance the glacier

descends; but in the case of the Antarctic ice-cap, the slope of the ground does not enter as an element into our calculations, for the ground is assumed to have no slope, the continent being regarded as flat. The surface no doubt may have great irregularities, such as hills and mountain ridges; those irregularities, however, do not assist gravity, but rather act as obstructions to the general flow of the ice.

Nevertheless, just as in the case of a glacier, the amount of work that gravity can perform is determined by the distance the ice can descend; and this distance is determined not by the slope of the ground, but by the thickness of the sheet. If the Antarctic ice-sheet be 1400 feet in thickness the greatest distance to which a pound of ice can descend is of course 1400 feet. Gravity acting on this pound of ice can therefore perform only 1400 foot-pounds of work. But, in order that gravity may do so, the pound must descend the whole distance from the surface to the bottom of the sheet. In estimating the total amount of heat which could possibly have been conferred on the ice by gravity, we must find the mean vertical distance to which the ice has descended. This of course, in the present case, is equal to half the thickness of the sheet, viz., 700 feet. If 1400 feet, as Sir Wyville Thomson supposes, be the thickness of the Antarctic ice-cap, 700 foot-pounds per pound is the utmost quantity of work that gravity can have performed on the ice. Supposing the whole of this work had been employed in heating the ice by compression, or by the friction of the particles of the ice on one another, or on the rocky floor of the sheet, the heat generated would not have amounted to one thermal unit per pound of ice. The specific heat of ice being about one-half that of water, the total work of compression—assuming that it had all been converted into heat, and the heat equally distributed through the entire mass of the cap—would not have raised the temperature of the ice by 2°.

The foregoing considerations do not afford a means of determining what the actual temperature of the great mass of the ice below the surface is. They show, however, that whatever that temperature may be it is not very materially affected either by the heat of compression, or by underground heat, or by that transmitted from the surface either by conduction or by melted ice.

On what, then, does the temperature of the ice mainly depend?

Temperature of the Ice determined by the Temperature of the Surface.—The temperature of the great mass of the ice is

mainly determined by the mean temperature of the upper surface of the sheet. All the ice down to the bottom of the sheet originally came from the surface. It once existed a the surface in the form of a coating of snow, which, be coming consolidated into ice, was afterwards covered over with fresh layers of snow, while these in turn passing into ice, were buried under succeeding snows, and so on. The ice that formed the surface a century ago now lies buried below the ice of a hundred years, and a hundred years hence its present position will be occupied by the surface ice of to-day. There is not only a constant motion of the ice from the pole outwards, but a constant downward motion as layer by layer is successively formed on the surface.

From what has been proved regarding the small quantity of heat which can be directly transmitted through the ice, it follows that the superficial layers will carry down with them pretty much the same temperature which they possessed at the surface at the time when they were covered up by succeeding snows. Any heat which they can derive from the work of compression, as has been shown, is but trifling. Heat transmitted by conduction could not possibly raise the temperature of the underlying ice above that of the surface; neither could the heat from direct radiation, nor that derived from melted ice.

As the temperature of the ice, then, cannot be much above the mean temperature of the surface, which is far below the freezing-point, it follows that the underlying mass must also be below the freezing-point. The very low temperature of the superficial layers is due to the fact that the mean temperature of the air above the surface is far below the freezing-point—a temperature which the icy surface cannot much exceed. The sun during summer may possibly heat the air sometimes above the freezing-point, but it cannot, of course, so raise the temperature of the ice without melting it.

Again, as solid ice is a better radiator than gaseous air, the surface of the sheet during winter would probably have its temperature lowered by radiation to a greater extent than the air. The probability is that the mean annual temperature of the surface is as low as, if not lower than, that of the air over it. And although the mean temperature of the regions around the South Pole has not been ascertained by direct observation, yet it certainly cannot be higher, but is probably much lower* than, that of those around the North Pole, which we know is but a few degrees above zero, F.

* See Climate and Time, p. 63.

Now if the mean annual temperature of the air over the Antarctic ice-sheet be about zero, F., then that of the surface of the sheet cannot be much higher; and if this be so, it follows, from what has been already advanced, that the temperature of the great mass of the ice down to near the bottom of the sheet must be considerably below the freezing-point.

Temperature of the Ice in some Regions determined by Pressure.—In regions such as Switzerland, where the mean temperature is above the freezing-point, the temperature of the ice in the interior of a glacier is not determined by the air above. The tendency of the air in this case is to keep the entire mass of the glacier at the melting-point. But as the temperature of the melting-point depends upon the pressure, the temperature of the glacier at any depth from the surface will depend upon the pressure to which the ice at that depth is subjected. At and near the surface, where the pressure is small, the temperature of the ice will be 32°. At the depth of a quarter of a mile, where the pressure would be equal to about 36 atmospheres, the temperature would be, not 32°, as at the surface, but 31°.5. And if the glacier were half a mile thick the temperature at the bottom would be 31°, and so on in proportion to the thickness of the glacier. This lowering of the melting-point could not, however, go on without limit, for in a country like Switzerland a point would soon be reached where the ice could no longer retain the solid form. If, for example, owing to the heat of the climate, we could not have ice at a lower temperature than say 30°, then a glacier over 1 mile in thickness would be an impossibility, for the bottom of a glacier of greater thickness would not remain solid at that temperature.

Having considered the various circumstances affecting the temperature of the Antarctic ice, and the sources from which it derives its heat, we have found that the temperature of the ice must be considerably under the freezing-point. We are now prepared to examine the reasons which have been adduced for concluding that about 1400 feet is the probable limit to the thickness of the Antarctic ice-sheet.

Examination of the Reasons for supposing that the Antarctic Ice-cap is not of Enormous Thickness.

1. *Limit to the Thickness of the Ice resulting from Melting produced by Pressure.*—Pressure will produce a melting of the ice in two totally different ways, viz., either by lowering the melting-point or by the work of compression. I shall consider both cases, and see if any ground is afforded by either in support of the conclusion that the Antarctic ice can be only one or two thousand feet in thickness.

(*a.*) *Melting produced by the Lowering of the Melting-point.*—The pressure exerted by a column of ice of half a mile in height would, as we have seen, lower the melting-point 1°; consequently, if the column were at the temperature of 32°, its base, being 1° above the melting-point, would not remain in the solid condition. To prevent the ice melting, the temperature of the base would require to be as low as 31°. Therefore, if 32° were the temperature of the Antarctic ice, Sir Wyville Thomson's conclusion, that the sheet cannot be more than 1400 feet in thickness, would follow as a matter of course.

But his supposition that, owing to internal heat coming through the earth's crust, the bottom of the Antarctic ice-sheet is kept at the temperature of 32°, cannot be sustained. It is this fundamental error, as I conceive it to be, which has led Sir Wyville astray, and induced him to believe that the ice cannot be of excessive thickness.

"The normal temperature of the crust of the earth," says Sir Wyville, "at any point when it is uninfluenced by cyclical changes, is, at all events, above the freezing-point, so that the temperature of the floor of the ice-sheet would certainly have no tendency to fall below that of the stream which was passing over it. . . . In fact, ice at the temperature at which it is in contact with the surface of the earth's crust within the Antarctic regions, cannot support a column of itself more than 1400 feet without melting."

In the question under consideration we are directly concerned with the temperature of the *surface* of the earth's crust only,—the floor on which the ice-sheet rests,—and not with the temperature below the surface. It is perfectly true that at considerable depths below the surface internal heat maintains a temperature above the freezing-point; but it is not true that it does so in reference to the surface. Under-

ground heat produces scarcely any sensible influence on the temperature of the surface, which is determined almost wholly by that of the air and other external agencies. The temperature, in short, is determined from *above*, and not from *beneath*. In warm countries, where the temperature of the air is high, that of the surface is high also. And so likewise in cold climates, the low temperature of the air gives a comparatively low temperature to the surface. Suppose our globe to be enveloped for some thousands of years with a covering at the uniform temperature of say 100°; and suppose, further, that 5000° should represent the temperature of the earth's mass; then, in such a case, there would be a gradual decrease of temperature from 5000° at the centre to 100° at the surface. Let us suppose now the warm covering is removed, and replaced by one at −100°. In the course of some thousands of years there will be a gradual decrease of temperature from 5000° at the centre, as before, to −100° at the surface. *Internal* heat limits the temperature at the centre, but *external* heat limits in every case the temperature at the surface.

To maintain, as Sir Wyville Thomson does, that 32° is the temperature of the floor on which the Antarctic ice-sheet rests, is virtually to beg the whole question at issue. It is the temperature of the ice that determines that of the floor on which it rests, and not the latter that determines the former.

What the temperature of the ground under the Antarctic ice-sheet may be is a question which at present we have no means of determining with certainty; we know only that it must be far below the freezing-point, for the ice resting on it is considerably under that point.

Although the temperature of the ice must impose a limit to the thickness of the sheet, underground temperature cannot do so, for the temperature of the ice is not determined by underground heat.

But supposing we knew the temperature of the Antarctic ice, yet this knowledge would not enable us to determine with certainty the limit imposed by temperature on the thickness of the ice. For, excepting in cases where the temperature is but very little below 32°, we are at present, in the absence of experiments, unable to say what would be the amount of pressure necessary to lower the melting-point to any assigned temperature. The experiment of Mousson shows that Prof. Thomson's formula, $t = 0{\cdot}0137°n$, does not hold true when the pressure is excessively great. A pressure of 73 atmospheres will lower the melting-point from 32° to

31°; but if Mousson's experiment is to be depended upon, a pressure of 400 atmospheres would be necessary to lower the melting-point from 1° to 0°. That is to say, were sufficient pressure applied to lower the melting-point to 1°, it would require an additional 400 atmospheres to lower it to 0°. The *rate* at which the melting-point is lowered by pressure is evidently not uniform, but decreases with the increase of pressure. Were the temperature of the ice at the South Pole as low as 32° below the freezing-point, which doubtless it is not, it would according to Mousson's experiment support a thickness of not less than 90 miles. But if the rate did not diminish with the pressure, but remained uniform, a pressure of 16 miles would be the limit.

From what has already been proved I think we may safely assume that the ice at the South Pole may be at least ten or twelve degrees below the freezing-point. We are unable to say what thickness of ice this temperature could support, but we know that it must be not under 6 nor over 30 miles.

But whatever the actual temperature of the Antarctic ice may be, if the sheet be as thick as the temperature will admit, then underground heat can never raise the temperature of the surface under the sheet sensibly above that of the ice. This is evident, because it cannot raise the temperature of the ice above the melting-point corresponding to the pressure, and the ice will always keep the floor at sensibly the same temperature as itself. In short, in determining the thickness of the Antarctic ice, underground heat does not enter as an element into our calculations, and, so far as the melting of the ice produced by the lowering of the melting-point is concerned, the Antarctic ice at the Pole may be a dozen of miles in thickness as readily as 1400 feet.

(*b.*) *Melting produced by Work of Compression and Friction.*—"The pressure upon the deeper beds of ice," says Sir Wyville Thomson, "must be enormous; at the bottom of an ice-sheet 1400 feet in thickness it cannot be much less than a quarter of a ton on the square inch. It seems therefore probable that, under the pressure to which the body of ice is subjected, a constant system of melting and regelation may be taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and finally working out channels for itself between the ice and the land, whether the latter be subaërial or submerged."

I need hardly state, that no amount of pressure, however great, has the least tendency whatever to produce a melting of the ice by heat unless this pressure performs work, and the quantity of ice melted will then be not in proportion to the pressure, but to the work performed by the pressure. The pressure here referred to, which is supposed to produce the melting, is the *weight* of the ice, or, in other words, the force of gravity.

When considering the amount of heat derived from work of compression it was proved that, in the case of the Antarctic ice-sheet, the total amount of work which can possibly be performed by gravity is determined by the thickness of the sheet. It was shown that if 1400 feet be the thickness of the sheet, 700 foot-pounds per pound of the sheet is the greatest amount of work that gravity can perform. It follows therefore that, supposing the whole of the work is employed in heating the ice by compression and friction the heat thus generated would amount to only 0·9 of a thermal unit per pound of ice. It must be obvious that in the case of a flat and tolerably uniform sheet like the Antarctic, in which the pressure must of course be pretty evenly distributed, little or no melting can take place from this cause, as it requires not 0·9 of a thermal unit, but 142 thermal units, to melt a pound of ice already at the melting-point. The total work of 158 pounds of ice would need to be concentrated upon 1 pound in order to melt it. But such an unequal distribution of force in a sheet so uniform is at least extremely improbable. The tabular form of the southern icebergs, with their stratification parallel to their upper surface, shows the flat character of the ground on which they have been formed. This circumstance appears to have particularly struck Sir Wyville Thomson, as well as all who have visited the Antarctic regions. "The stratification," says Sir Wyville, "in all the icebergs which we saw was, I believe, originally horizontal and conformable, or very nearly so. I never saw a single instance of deviation from the horizontal and symmetrical stratification which could in any way be referred to original structure. . . . . As I have already said," he continues, "there was not, so far as we could see, in any iceberg, the slightest trace of structure stamped upon the ice in passing down a valley, or during its progress over *roches montonnées*, or any other form of uneven land; the only structure, except the parallel stratification, which we ever observed which could be regarded as bearing upon the mode of original formation of the ice-mass was an occasional local thinning out of some of the layers and

thickening of others,—just such an appearance as might be expected to result from the occasional drifting of large beds of snow before they have time to become consolidated."

The comparative absence of stones, gravel, or earth on the southern icebergs shows likewise the flat nature of the Antarctic ice-covering. "We certainly never saw," says Sir Wyville, "any trace of gravel or stones, or any foreign matter necessarily derived from land, on an iceberg."

But supposing we should make the extravagant assumption that in this comparatively flat and uniform sheet the pressure by some unexplained means is not evenly distributed, but that, on the contrary, it is all brought to bear on certain points and consumed in melting the ice, and that the total quantity of ice melted is the exact equivalent of the work performed by gravity; and let us further assume that the entire mass of the ice is already at the melting-point, and that, therefore, no work is required to raise its temperature; then the total quantity of ice melted would be of course $\frac{9}{142}$ or $\frac{1}{158}$ of the entire mass. Gravity could perform only $\frac{1}{158}$ of the amount of work required to melt the entire sheet. If we suppose the sheet to be 1400 feet thick, then $\frac{1}{158}$ of this thickness will be equal to 9 feet. A layer of ice about 9 feet in thickness, therefore, is the total amount that gravity could under any circumstances have melted.

But more than this, it must be borne in mind that these 9 feet represent the total quantity which could be melted during the whole time the sheet was being formed; that is, from the time the bottom layer fell in the form of snow on the surface down to the present day. We have no means of ascertaining the length of this period. If we assume it to be 10,000 years, and this is probably an underestimate, then 9 feet of ice melted during that period would amount to only 1 inch in ninety-two years, or $\frac{1}{92}$ of an inch annually. But whether the period be 10,000 or 5000 years the quantity is so trifling that it may be practically disregarded in the present inquiry.

Nor is this all, for if the great mass of the ice be as much as 2° below the freezing-point, which it undoubtedly is, the total amount of heat generated by compression and friction during the 10,000 years would not suffice to raise the temperature of the ice even to the melting-point.

The Great Diminution in the Thickness of the Ice-strata from the Top downwards not due, as supposed, either to Compression or to Melting.—The thinness of the lower as compared with the more superficial strata of the ice-sheet is considered by Sir Wyville Thomson to be mainly, if not altogether, due to two causes, Compression and Melting of the Ice, particularly the latter. "The regularity of this diminution," he says, "leaves it almost without a doubt that the layers observed are in the same category, and that therefore the diminution is due to subsequent pressure or other action upon a series of beds which were at the time of their deposition pretty nearly equally thick. About 60 or 80 feet from the top of an iceberg the strata of ice are a foot or so in thickness, although of a white colour, and thus indicating that they contain a quantity of air and that the particles of ice are not in close apposition, are still very hard, and the specific gravity of the ice is not very much lower than that of layers not more than 3 inches thick nearer the water-line of the berg. Now it seems to me that this reduction cannot be due to compression alone, and that a portion of the substance of these lower layers must have been removed."

If the layers 3 inches thick near the water-line were once a foot in thickness, as no doubt they were, then this great diminution in thickness cannot have been due to compression, for had it been so the density of those layers would be more than double that of water. But Sir Wyville has found that the specific gravity of the layers 3 inches thick is not much lower than of those a foot in thickness, which proves, as he has pointed out, that compression cannot account for their thinness; but it does not, as we shall presently see, necessarily prove "that a portion of the substance of these lower layers must have been removed."

Assuming that the lower layers were all originally of the same thickness as the upper, it can nevertheless be shown that the gradual diminution in thickness of the layers from the top downwards follows independently altogether of compression or of the removal of any portion of the substance of the layers, either by melting or by any other means.

Ice radiating from a Centre of Dispersion becomes thinner, because the space over which it is spread becomes greater.—There is this peculiarity in Continental ice, that there is a centre of dispersion from which the ice radiates in all directions. This is particularly true in reference to the Antarctic ice-cap. It does not necessarily follow that the centre of

dispersion is the centre of the sheet. In the case of the Antarctic sheet the centre of dispersion cannot, however, be far from the Pole, and the Pole in all probability is not far from the centre of the sheet. We may therefore in our inquiry safely assume the Pole to be the centre of dispersion. It is obvious that, if the Antarctic ice be radiating in all directions from the Pole as a centre, a portion of a layer which in say latitude 85° covers 1 square foot of surface will on reaching latitude 80° cover 2 square feet. At latitude 70 it will occupy 4 square feet, and at latitude 60° the space covered will be 6 square feet. Then if the layer was 1 foot thick at latitude 85°, it would be only 6 inches thick at latitude 80°, 3 inches thick at latitude 70°, and 2 inches at latitude 60°. Had the square foot of ice come from latitude 89° it would occupy 30 square feet by the time it reached latitude 60°, and its thickness would be reduced to 1-30th of a foot, or 2-5ths of an inch.

Now the lower the layer the older it is, and the greater the distance which it has travelled. A layer near the bottom may have been travelling from the Pole for the past 10,000 or 15,000 years, whereas a layer near the top may perhaps not be twenty years old, and may not have travelled the distance of a mile. The ice at the bottom of a berg may have come from near the Pole, whereas the ice at the top may not have travelled 100 yards. It follows therefore that, other things being equal, the lower a layer is the thinner it should be, and that this is perfectly sufficient to account for the decrease in the thickness of the layers from the top downwards, without assuming any of the ice to have been removed by melting or by any other means.

Continental Ice radiating from a Centre of Dispersion must be thickest at the Centre, and gradually diminish in Thickness towards the Circumference.—Whatever theory we may adopt as to the cause of the motion of ice, it will follow as a necessary consequence that the sheet must be thickest at the centre and thinnest at its edge. In a continental sheet like that covering the Antarctic regions we are not warranted, as has already been noticed, in assuming that the surface of the ground under the sheet slopes persistently outwards from the centre or Pole to the edge; in other words, we cannot infer that the Antarctic ice, like an ordinary glacier, rests on an inclined plane.

Now if we adopt the generally accepted theory, that Gravity is the force impelling the ice forward, we must assume the sheet to be thickest at the centre; for unless it

were so gravity could have no tendency to produce motion, because the force which moves the ice must not only act horizontally, but act more in one direction than in another; and this it could not do were the ice of uniform thickness. Were the sheet of this uniform thickness the forces acting on it would balance each other, and no motion could result. If the sheet is to be forced out horizontally along the flat surface by *its own weight*, then there must be a piling up of the ice in the interior. If the ice comes from the centre, then the pressure must be greatest there; but in order to this the sheet, of course, must be thickest at the centre.

Supposing it should be asserted that it is not the pressure of the particle *a* that moves the particle *b* in front of it, and the pressure of the particle *b* that moves the particle *c*, and so on, but that each particle moves by its own weight, we are nevertheless led to the same conclusion. The weight of the particle (the force of gravity) will not move the particle unless the particle is allowed to descend. If a particle moves by its own weight from the centre of the sheet to the circumference, it must *descend:* it must pass from a *higher* to a *lower level*. It must move down an inclined plane from the centre to the circumference, but to allow it to do so the sheet must be thickest at the centre.*

If, on the other hand, we adopt the "Molecular" theory, or the "Dilatation" theory of the motion of the ice, or any other theory whatever which attributes the motion of the ice not to gravity, but to some expansive force acting in the interior of the mass, we are equally led to the same conclusion as to the greater thickness of the sheet at the centre. Although such a force will of course tend to push the ice as powerfully inwards in the direction of the Pole, or centre, as outwards in the direction of the circumference, yet the motion of the ice will always take place in the latter direction, and never in the former, for the latter will always be the direction of least resistance. The tendency of such a force is to produce an outward motion of the ice on the outer side, but to hold back or prevent such a motion taking

* That the entire mass of the Antarctic ice down to the bottom is in a state of motion, and not simply the upper layers, as some suppose, is demonstrable from the fact that icebergs are stratified down to their base. The iceberg is simply a piece broken off the edge of the sheet, and the stratified face of the berg is the counterpart of the edge from which it broke off; and as the icebergs are known to be stratified to their base, it proves that the sheet from which they were derived is likewise stratified to the bottom. The fact, therefore, that stratified icebergs are continually breaking off the Antarctic sheet, and have been for ages, proves that the sheet down to its bottom must have been in a state of outward motion.

place in the ice on the inner or poleward side. As such an expansive force is assumed to act in every portion of the mass, it follows that the nearer the outside of the sheet the more rapidly will the ice move, and consequently the thinner will the sheet become.

The Greater Thickness of the Sheet at the Pole independent of the amount of Snowfall at that place.—It has been proved that unless the Antarctic ice were thickest at the pole and thinnest at the edge, motion could not take place. It follows, therefore, that however much the snowfall at the edge and other places may exceed that at the Pole, or centre of dispersion, the ice must always be thickest at the centre. For however small may be the snowfall, and consequent amount of ice formed annually at the Pole, snow and ice must of necessity continue to accumulate year by year till the sheet becomes thickest there. The ice at the Pole could not move out of its position till this were the case. Supposing there were no snow whatever falling at the Pole, and no ice being formed there, still the sheet would be thicker there than at the edge. For in this case the ice forming at some distance from the pole all around would flow back towards the centre, and continue to accumulate there till the resistance to the inward flow became greater than the resistance to the outward; but this state would not be reached till the ice became thickest on the poleward side.

We have no reason to believe, however, that the quantity of snow falling at the Pole is not great. "One thing we know," says Sir Wyville Thomson, "that the precipitation throughout the Antarctic area is very great, and that it is always in the form of snow." Lieut. Wilkes, of the American Exploring Expedition, estimated the snowfall to be 30 feet per annum, and Sir James Ross says that during a whole month they had only three days free from snow. The very fact that perpetual snow is found at the sea-level at lat. 64° S. proves that the amount of precipitation in the form of snow in those regions must be great.

But there is one circumstance which must tend to make the snowfall near the Pole great, and that is the inflow of moist winds in all directions towards it; and as the area on which these currents deposit their snow becomes less and less as the Pole is reached, this must, to a corresponding extent, increase the quantity of snow falling on a given area. Let us assume, for example, that the clouds in passing from lat. 60° to lat. 80° deposit moisture sufficient to produce, say, 30 feet of snow per annum, and supposing that by the time they reach lat. 80° they are in

possession of only one-tenth part of their original store of moisture,—still, as the area between lat. 80° and the Pole is but one-eighth of that between lat. 60° and 80°, this would notwithstanding give 24 feet as the annual amount of snowfall between lat. 80° and the Pole.

Rate of Motion of the Antarctic Ice.—If we knew the rate at which the edge of the Antarctic ice-cap is advancing outwards, we could form a rough estimate of the amount of snowfall on the continent. Or, conversely, knowing the amount of snowfall, we could tell approximately the rate at which the ice is moving outwards.

Dr. Rink calculates that the yearly precipitation on Greenland in the form of snow and rain amounts to about 12 inches. About 2 inches he considers is carried off by ice into the sea, and the remaining 10 inches is carried to the sea in the form of sub-glacial rivers. He believes that the quantity disposed of by evaporation is trifling.

The amount of precipitation on the Antarctic continent is probably much greater than on Greenland. On the Antarctic continent it is all in the form of snow or hoar-frost, whereas in Greenland a considerable portion of it—in summer at least—is in the form of rain. For reasons already stated the proportionate amount carried off the Antarctic continent in the form of water to that of ice must be much less than on Greenland. The quantity of ice melted in the Antarctic regions from all causes, we have seen, cannot be great; and of that quantity the greater part must be re-solidified long before it can reach the sea. I can hardly think that it will be regarded as an over-estimate to affirm that at least one-half the precipitation must reach the sea in the form of ice. Assuming the annual precipitation to be no greater than that of Greenland, viz., 1 foot per annum, the quantity carried off in the form of ice would in this case be 6 inches. At what rate, then, would the edge of the cap require to be advancing outwards in order to discharge this 6 inches of ice? If we assume the cap to extend on an average down to latitude 70°, its area will be about 5,940,000 square miles, or 165,611,000,000,000 square feet. A layer 6 inches thick covering that area would contain 82,805,500,000,000 cubic feet of ice. The circumference of the cap is 45,300,000 feet, and its thickness at the edge is assumed of course to be 1400 feet. Were the ice, therefore, to move outwards at the rate of 1300 feet per annum, and to break up into bergs as it advanced, the quantity of ice discharged annually in

the form of icebergs would be 82,446,000,000,000 cubic feet, an amount equal to the layer of ice 6 inches in thickness covering the area. Consequently, if 6 inches of ice be carried annually off the Antarctic continent, the edge of the cap must be moving outwards at the rate of about a quarter of a mile annually. Even supposing there were only 2 inches of ice discharged, the rate of motion would require to be between 400 and 500 feet per annum.

A quarter of a mile per annum cannot be regarded as an improbable rate of motion for continental ice, when we reflect that the Greenland ice has in some places a velocity ten times greater. Mr. Amund Helland, for example, found that the glacier of Jakobshaven has a velocity of about 20 metres *per diem*, which is upwards of 4 miles annually. The exceptional high velocity of the Greenland glaciers is no doubt owing to the fact that the ice-sheet covering that continent has to force its way through comparatively narrow outlets. If the sheet moved off the land in one unbroken mass, like the Antarctic sheet, its rate of motion would be much less.

It is the immense extent of the Antarctic continent which demands such a high velocity to get rid of the ice. To enable it to discharge the annual amount of ice, either the sheet must be excessively thick or its rate of motion excessively great. If, for example, the ice were only 700 feet instead of 1400 feet thick, its motion would require to be half a mile annually in order that the 6 inches of ice should be got rid of; while, if it were only 100 feet in thickness the rate of motion would need to be $3\frac{1}{2}$ miles per annum.

It is this difficulty in getting away which is the chief cause of the enormous accumulation of ice on the Antarctic continent. And it is just this great thickness in the interior that enables the sheet to get rid of its superabundant ice. This is effected in two ways:—1st. The greater the thickness of the ice in the interior, the greater is the force by which it is impelled outwards, and, other things being equal, the greater is the velocity of the ice. 2nd. The thicker the sheet becomes, the greater is the quantity discharged corresponding to a given velocity. The velocity being the same, the quantity discharged is in proportion to the thickness of the sheet.

With the present rate of snowfall on the Antarctic continent it is physically impossible that the ice can be otherwise than of great thickness. Were not the sheet enormously thick the quantity of ice annually discharged would not equal that being formed, and consequently the ice would

of necessity increase in thickness year by year, till the rate of discharge became equal to that of growth. We have just seen that it would require a thickness of not less than 1400 feet at the very edge of the cap to make the two rates equal, even although the ice was moving outwards with a velocity of a quarter of a mile per annum, a rate of motion greater than that of an Alpine glacier; and, on the other hand, to produce such a rate of motion as this, a thickness in the interior enormously greater than 1400 feet is required. If, from an increase in the snowfall, or from a decrease in the quantity of snow and ice melted, or from both combined, the annual amount of ice requiring to be discharged were doubled, the velocity remaining the same, the thickness of the sheet would ultimately become doubled also. Or, if the thickness of the sheet remained the same the velocity would be doubled. The actual result in such a case, however, would be that a restoration of equilibrium between supply and discharge would take place, by an increase both of thickness and velocity. As the quarter of a mile per annum of velocity would only be sufficient to discharge one-half the amount of ice being formed, the sheet would increase in thickness year by year. But this increase in thickness would produce an increase of velocity, and the increase both in thickness and velocity of motion would continue till the quantity of ice discharged would be equal to the 12 inches over the whole area, instead of the 6 inches as before. Equilibrium being now established, no further increase would take place either in the thickness of the sheet or in the velocity of its motion. If, on the contrary, the amount of ice being formed on the Antarctic continent were to become less than at present, both the thickness of the sheet and the velocity of its motion would become less.

The following conclusions have now been established:—

1. The Antarctic ice-sheet must be thickest at the centre of dispersion and thinnest at the edge.

2. The rate of motion of the ice must be least at the centre of dispersion and greatest at the edge.

3. The mean thickness of the edge of the sheet, other things being equal, must be proportional to the area of the sheet, and inversely as the rate at which the edge is moving outwards.

4. The area of the sheet, the thickness of its edge, the velocity of its motion outwards, the amount of snow-fall, and the temperature of the climate are so related to one another that the value of any one of them can be determined approximately in terms of the rest.

The probable Thickness of the Ice at the Pole.—The point which now remains to be determined is, What is the thickness of the ice at the Pole, or centre of dispersion? The thickness of the sheet at the edge is admitted to be about 1400 feet, and this, as has been demonstrated, must be the thinnest part of the sheet. It must gradually thicken inwards towards the Pole as centre of dispersion, where the thickness becomes a maximum. How much thicker, then, must the sheet be at the centre than it is at the circumference? The question to be determined, stated in another form, is, What is the thickness of ice at the Pole required in order to impel the cap outwards in all directions at the rate of a quarter of a mile per annum, or even half that rate per annum? The upper surface of the sheet must slope upwards towards the centre or pole. What is the amount of this slope?

The Antarctic continent is generally believed to extend on an average from the South Pole down to about, lat. 70° or so. In round numbers we may take the diameter of the continent at 2800 miles. The distance from the edge of the ice-cap to its centre, the Pole, will therefore be 1400 miles. A slope of 1 degree, continued for 1400 miles, will give 24 miles as the thickness of the ice at the Pole. But would a slope of 1 degree be sufficient to produce the required amount of motion? If the generally accepted theory of the cause of glacier motion be correct, it certainly would not. But supposing we assume that one-half or even one-quarter that amount of slope would suffice, still we have 6 miles as the thickness of the cap at the Pole.

To those who have not been accustomed to reflect on the physical conditions of this problem, this estimate may doubtless be regarded as somewhat extravagant; but a slight consideration will show that it would be even more extravagant to assume that a slope of less than half a degree would be sufficient to produce the necessary outflow of the ice. In estimating the thickness of a sheet of continental ice of one or two thousand miles across, our imagination is apt to deceive us. We can easily form a pretty accurate sensuous representation of the thickness of the sheet, but we can picture to ourselves no adequate representation of its superficial area. We can realise with tolerable accuracy a thick-

ness of a few miles, but we cannot do this in reference to a superficial area 2800 miles across. Consequently, in judging what proportion the thickness of the sheet should bear to the superficial area, we are apt to fall into the error of under-estimating the thickness. We have a striking example of this in regard to the ocean. That which impresses us most forcibly in regard to the ocean is its profound depth. A mean depth of (say) 3 miles produces a striking impression; but if we could represent to the mind the vast area of the ocean as correctly as we can do its depth, *shallowness* rather than *depth* would be the impression produced. A sheet of water 100 yards in diameter, and only 1 inch deep, would not be called a *deep* but a very *shallow* pool or thin layer of water. But such a layer would be a correct representation of the ocean in miniature. Were we, in like manner, to represent to the eye in miniature the Antarctic ice-cap, even as 12 miles in thickness at the Pole, we should call it a *thin crust of ice.* The mean thickness of the sheet would be about 4 miles, and this would be represented by a carpet covering the floor of an ordinary-sized dining-room. Were those who consider the above estimate of the Antarctic ice-cap as extravagantly great called upon to sketch on paper a section of what they should deem a cap of moderate thickness, ninety-nine out of a hundred would draw one of much greater thickness than 12 miles at the centre.

The accompanying diagram represents a section across the cap drawn to a natural scale, the upper surface of the sheet having a slope of half a degree. No one looking at the section would pronounce it to be too thick at the centre unless he were previously made aware that it represented a thickness of 12 miles at that place. The section, of course, is not intended to represent the actual thickness of the sheet, but to show how liable we are to over-estimate a thickness proportionate to an area so immense. It may here be mentioned that had the section been drawn upon a much larger scale —had it, for instance, been made 7 ft. long instead of 7 inches—it would have shown to the eye in a more striking manner the thinness of the cap.

SECTION ACROSS ANTARCTIC ICE-CAP, DRAWN TO A NATURAL SCALE.

Length represented by section = 2800 miles. Thickness of centre (South Pole) = 12 miles.
Slope of upper surface = half-degree.

At the close of the reading of Dr. James Geikie's paper "On the Glacial Phenomena of the Long Island," before the Geological Society, in May last, His Grace the Duke of Argyll stated that he doubted whether ice could move on a slope of 1 in 211. But a slope so small as 1 in 211 would give a thickness of 7 miles at the Pole. Consequently we have no alternative but to admit that a slope of 1 in 211 is sufficient, or the cap must be over 7 miles thick at the Pole.*

But to avoid all objections on the score of over-estimating the thickness of the cap, let us assume that a slope of an eighth of a degree would be sufficient to produce the necessary motion; the thickness of the sheet would of course be one-fourth that represented in the diagram, *but still it would be 3 miles thick at the Pole!*

There is another cause which tends to mislead us in forming an estimate of the actual thickness of the Antarctic ice. It is not in consequence of any *à priori* reason that can be urged against the probability of such a thickness of ice, but rather because it so far transcends our previous experience that we are so reluctant to admit such an estimate. If we never had any experience of ice thicker than what is found in England, we should feel startled on learning for the first time that in the valleys of Switzerland the ice lay from 200 to 300 feet in depth. Again, if we had never heard of glaciers thicker than those of Switzerland, we could hardly credit the statement that in Greenland they are actually from 2000 to 3000 feet thick. We, in this country, have long been familiar with Greenland; but till very lately no one ever entertained the idea that that continent was buried under one continuous mass of ice, with scarcely a mountain top rising above the icy mantle. And had it not been that the geological phenomena of the Glacial epoch have for so many years accustomed our minds to such an extraordinary

* Dr. J. Geikie writes me as follows:—"I have given the height of the glaciation in the North-west Highlands as 3000 feet or thereabout, which, taken in connection with the glacial phenomena of the Outer Hebrides, implies a slope for the surface of the ice-sheet of 1 in 211, or about 25 feet in the mile. It is not improbable, however, that a more detailed examination of the mainlands may compel up to admit a still greater thickness for the ice-sheet of the North-west—the surface of which may have reached to a height of 3500 feet in Rosshire. This would yield a slope of 35 instead of 25 feet in the mile. After my paper had gone to press I received, through the kindness of Mr. George H. Cook, State Geologist of New Jersey, a copy of his Annual Report for 1877, in which the slope of the ice-sheet that flowed into the northern part of that State is estimated at 34 feet in the mile. Prof. Dana, you will remember, comes to the conclusion that the surface of the ice-sheet attained a height upon the Canadian water-shed of 12,000 feet, on the supposition that the ice sloped southwards at the rate of 10 feet in the mile,—if the slope were greater, the Canadian ice, of course, must have been thicker. The inclination of the ice-sheet in the area of the North Sea I estimate at about 12 or 13 feet in the mile."

condition of things, Dr. Rink's description of the Greenland ice would probably have been regarded as the extravagant picture of a wild imagination.

Greenland Ice-sheet.—There is no reason to conclude that the northern hemisphere possesses, or perhaps ever possessed, an ice-cap like the southern. Greenland, however, possesses on a smaller scale a condition of things similar to that of the Antarctic continent. The same general principles which we have been considering in relation to the Antarctic ice-sheet must hold true in regard to the ice of Greenland.

The Greenland ice-sheet, like the Antarctic, must be *thickest at the centre of dispersion and thinnest at the edge.* Greenland is more accessible than the Antarctic continent, yet—with the exception of Dr. Hayes and Prof. Nordenskjöld, and a small party under the command of Lieut. Jensen, R.D.N., recently sent out by the Danish Government—no one, so far as I am aware, has ever succeeded in penetrating to any great distance into the interior over the inland ice. Nevertheless, the state of things as recorded by these explorers affords us a glimpse into the condition of the interior of that continent. They all found by observation, what we have already seen follows as a necessary result from physical considerations, that the upper surface of the ice-plain under which hills and valleys are buried *gradually slopes upwards towards the interior of the continent.*

Professor Nordenskjöld states that at 30 geographical miles from the coast, the extreme point reached by him, he had attained an elevation of 2200 feet, and that the inland ice *continued constantly to rise* towards the interior, so that the horizon towards the east, north, and south was terminated by an ice-border almost as smooth as that of the ocean.*

Dr. Hayes and his party penetrated inwards to the distance of about 70 miles. On the first day they reached the foot of the great *Mer de Glace;* the second day's journey carried them to the upper surface of the ice-sheet. On the third day they travelled 30 miles, and the ascent, which had been about 6°, diminished gradually to about 2°. They advanced on the fourth day about 25 miles, with the temperature 30° below zero. "We had attained," he says, "an altitude of *five thousand feet above the sea-level,* and were 70 miles from the coast, in the midst of a vast frozen Sahara immeasurable to the human eye. There was neither hill, mountain, nor gorge anywhere in view. We had completely sunk the strip of land between the *Mer de Glace* and the sea, and no object met the eye but our feeble tent, which bent to the storm."†

* Geol. Mag., 1872, vol. ix., p. 360.
† Open Polar Sea, p. 134.

Dr. Rink, referring to the inland ice, says that the elevation or height above the sea of this icy plain, at its junction with the outskirts of the country, and where it begins to lower itself through the valleys to the friths, is, in the ramifications of the Bay of Omenak, found to be 2000 feet, from which level *it gradually rises towards the interior.**

Dr. Robert Brown is of opinion that Greenland is not traversed by any range of mountains or high land, but that the entire continent, 1200 miles in length and 400 miles in breadth, is covered with one continuous unbroken field of ice, the upper surface of which, he says, *rises by a gentle slope towards the interior.*†

Lieut. Jensen and his party, after great toil, succeeded in reaching a mountain 50 miles from the border of the ice-fields. The height of the mountain which they ascended was ascertained to be about 5000 feet above-sea-level. From the summit they found the ice-fields beyond stretching as far as the eye could see, the plateau apparently rising *higher and higher towards the interior.*

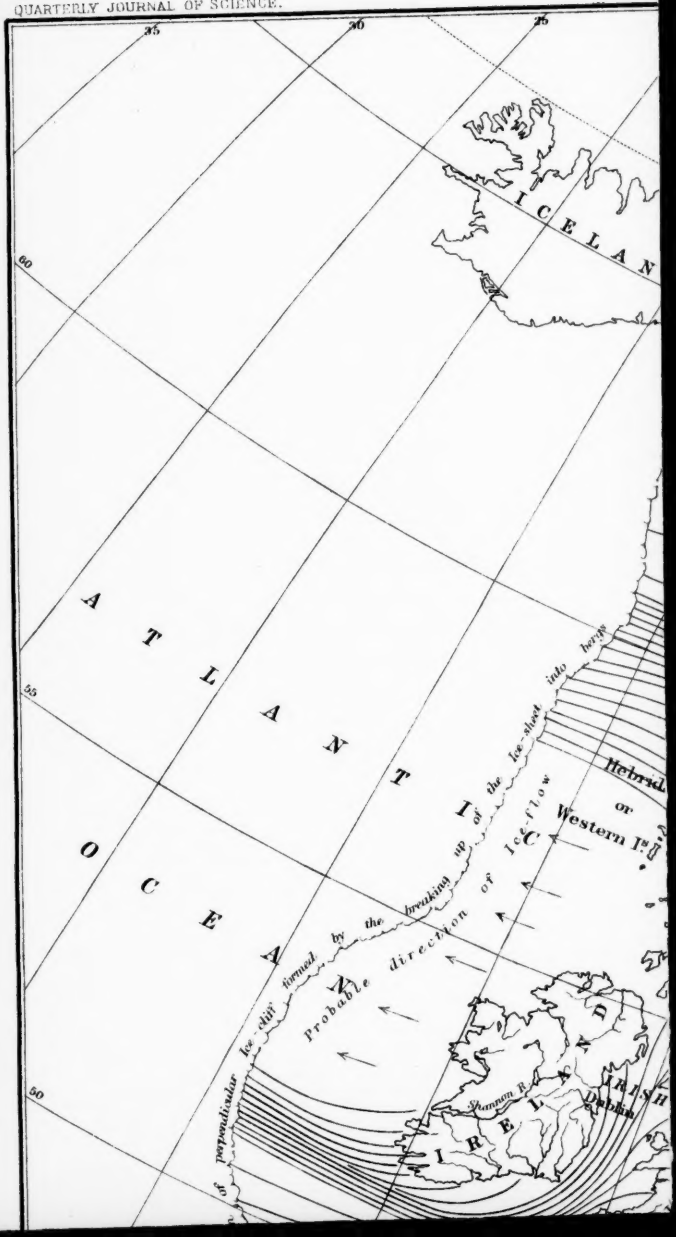
There is little doubt that an expedition into the interior of Greenland would throw more light on the physical nature of continental ice than one to the North Pole.

The Ice of the Glacial Epoch.—The same general principles which we have been considering hold equally true in reference to the ice of the Glacial epoch. Misapprehensions regarding the magnitude of continental ice lie at the very root of the opposition with which the Land-ice Theory of the chief phenomena of the Glacial Epoch has had to contend. One of the main objections urged against that theory is the magnitude of the ice-sheet which it demands. For example, to explain the glacial phenomena by the theory of land ice, we are compelled to infer that the whole of Scotland, Scandinavia, and the greater part of North-western Europe, were not only covered with ice, but covered to a depth of one or two thousand feet. But not only are the mainlands glaciated, but the islands of the Baltic, the Orkneys, the Shetlands, and the Hebrides, bear equal evidence of ice having passed over them. To explain this by the theory, we have further to assume that the ice-sheet which covered the land must have filled the Baltic, the German Ocean, and the surrounding seas; in short, that all these regions were buried underneath one continuous mass of ice.

To one with inadequate conceptions of the nature of con-

* Journal of the Royal Geographical Society, 1853, vol. xxiii.

† "Physics of Arctic Ice," Quart. Journ. Geol. Soc. for February, 1871.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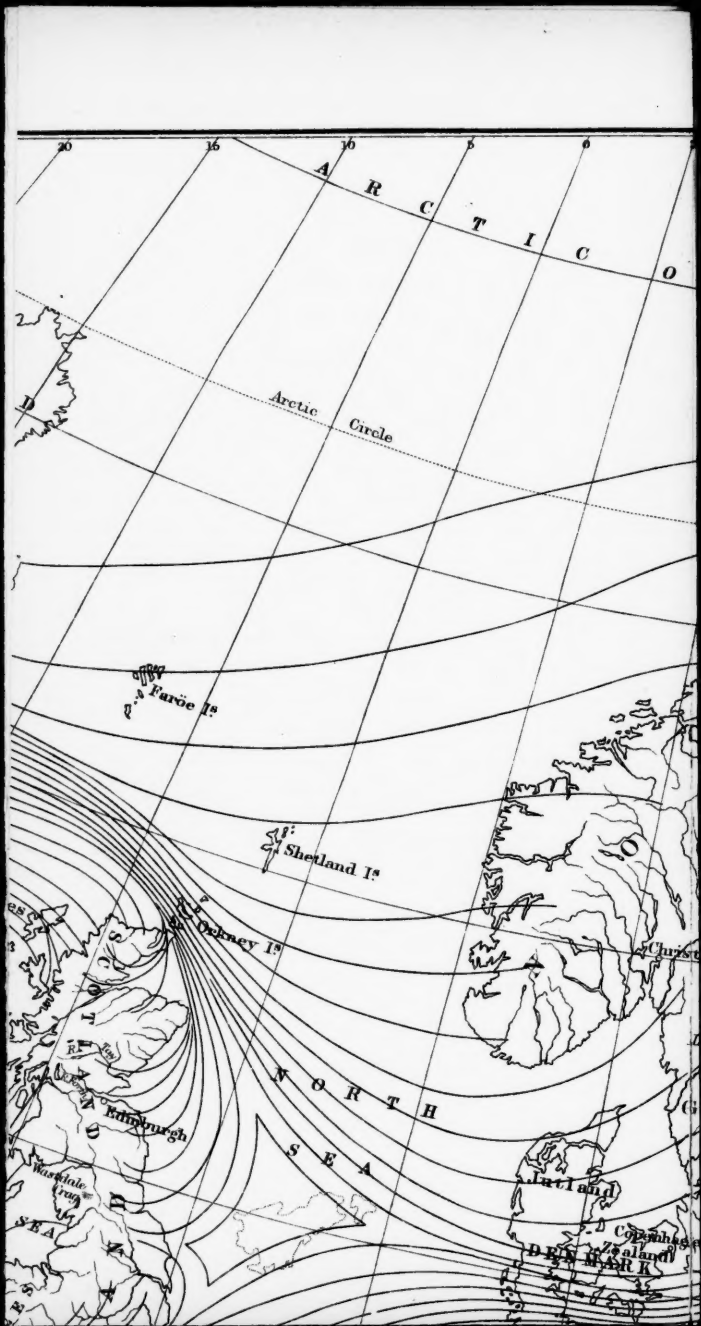
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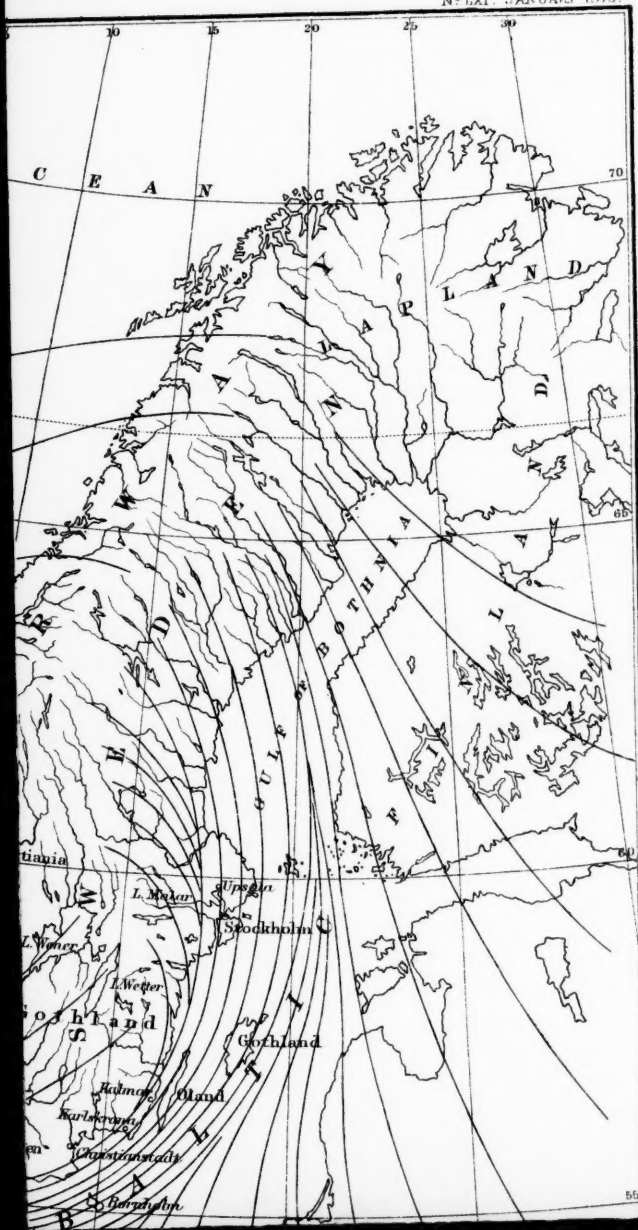
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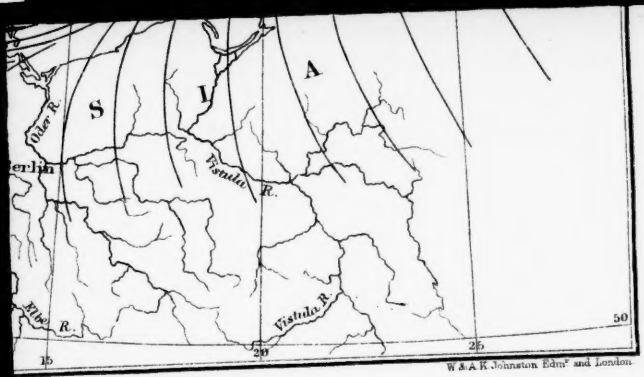

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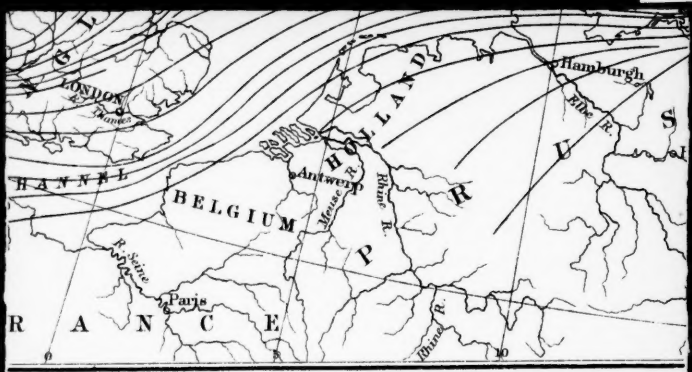

LE PATH OF THE ICE IN NORTH-WESTERN EUROPE DURING THE PERIC

The lines also represent the actual direction of the striae on the rocks.

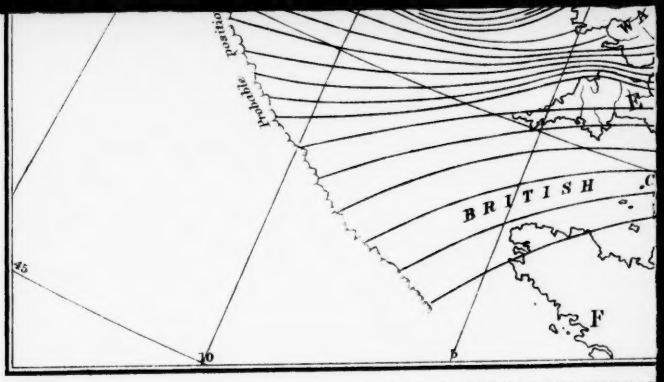

CHART SHOWING THE PROBAB

tinental ice such a condition of things as this may appear incredible; but if the principles we have been considering be correct, it follows as a necessary consequence. If, during the Glacial Epoch, the quantity of ice annually formed in North-western Europe was much in excess of the quantity melted, enormous ice-sheets must of necessity have been formed.

The thickness of the sheet or sheets covering that region would depend, as has been shown, upon the area covered and the rate of snowfall, or, rather, the rate at which the ice was being formed. The sheet, as has also been shown, must have been thickest at the centre or centres of dispersion—if there were more than one—and thinnest at the edge. The extent of area covered by ice on North-Western Europe must have been great; so also must have been the amount of snowfall. If we assume that the Glacial epoch was due to an increase in the eccentricity of the earth's orbit, it follows that the amount of snowfall on the ice-sheet, during summer at least, must have been excessive. Owing to the nearness of the sun in perihelion at that season, the amount of evaporation in the Atlantic would be greater than at present. The aqueous vapour thus raised would be carried north-eastward by the south-west winds, which would then —for reasons which have been stated at length elsewhere—be much stronger than they are at present. The vapour on reaching the ice-sheet would be condensed by the cold, and fall as snow. Owing to the rapid accumulation of the ice over so large an area, and the consequent difficulty in getting rid of it, a condition of things similar to that which now obtains in the Antarctic regions would ere long result.

That such a condition as this, to which we are led by theoretical considerations, did actually prevail during the Glacial epoch is now established by the facts of observation. Norway we know was the great centre of dispersion of the ice, and here it has been found that the sheet attained its greatest thickness. It has been shown by Mr. Amund Helland that its thickness there was over a mile. Scotland was also a subordinate centre of dispersion, and we know that the ice moving off it was sufficient to prevent that country from being overridden by the great mass of ice flowing outwards in all directions from the Scandinavian centre. It was sufficient, but little more, for the Scandinavian ice filling the German Ocean, and passing over the Orkney and Shetland Islands, was so powerful as to bend back the Scottish ice, and force it to turn round, after it had

entered the German Ocean, and pass obliquely over the flat lands of Caithness.*

The accompanying Chart, reproduced from "Climate and Time," shows the probable path taken by the ice in its outward motion from these two centres of depression. The Chart, it is to be observed, is not theoretical, for the lines represent the actual direction of the striæ on the rocks. It was inadequate conceptions of the nature of continental ice which prevented geologists from coming to the conclusion that all those striæ were produced by land ice.

II. GRAVITATION AS A FACTOR IN THE ORGANIC WORLD.

By William Crookes, F.R.S.

It cannot be denied that in discussing the great questions of the origin of life and the development of species,—in seeking to explain to ourselves why every creature is as we find it, and not otherwise,—we have to deal with two sets of factors.

On the one hand we have certain outward agencies, such as the great cosmic forces, the chemical and physical character of the earth's surface and of its liquid and gaseous envelopes, the varying effects of climate, the quality and quantity of food accessible, the conflict with other species,—everything, in short, which the animal encounters either as an enemy or as an ally in its struggle for existence.

On the other hand, there are doubtless inward tendencies upon which these external forces react. For if we see a roll of calico passing through a dye-bath of alizarin, and coming out not all of one uniform shade, but displaying a parti-coloured pattern, we know that different mordants must have been imprinted upon it with each of which the dye gives a different result. Just so here; were the outer factors alone present, no variety could result from their action. But of

* An important paper by my colleagues, Mr. B. N. Peach and Mr. J. Horne, on the Glacial Phenomena of the Shetland Islands, will shortly be read before the Geological Society of London. They have found evidence which puts the question beyond all doubt that the whole of those islands have been glaciated by land-ice from Scandinavia.

these inward factors we know as yet nothing. Nor can we even hope to begin their study till we have first traced out and eliminated the workings of the external agencies which are simpler in themselves, and whose general character we have had the opportunity of learning from the phenomena of inorganic nature. When this task is once accomplished we shall then find certain "residual phenomena" not to be accounted for by the outward factors above mentioned, and from these phenomena we may hope to argue back to their unknown and doubtless exceedingly complicated causes.

But very much still remains to be done before we can decide with certainty what part is played by such external forces.

The functions of electricity have indeed not been disregarded. In semi-scientific circles it fills the post which popular tradition once allotted alternately to Michael Scott and to the devil—doing duty, namely, as the originator of everything unaccountable. The influence of light and of heat upon organic beings has been investigated with much care, and though the task is far from complete, yet valuable results have been reached. Above all it has been established that certain limits of temperature exist, above and below which all our experience declares life to be impossible. All organisms, as we find them upon our globe, consist of a combination of solids and liquids. But below a certain degree of heat the liquid constituents become solidified, and thus are rendered incapable of playing their part in the vital process. If such solidification is temporary it may involve merely a suspension of animation. Above a certain temperature both liquids and solids would be either volatilised or resolved into their inorganic constituents; hence it follows that the chemical composition of every organism must stand in a certain relation to the thermic conditions of the world it inhabits. Very similar is the case as regards light, its total absence and its excess being alike unfavourable to the welfare of plants and animals.

But the effects of gravitation in its several manifestations seem scarcely to have received the attention which they merit. Yet that this all-embracing force must have left its impress upon the organic world, no less than upon lifeless matter, can scarcely require any formal demonstration.

For what is an animal? If we take as the type the highest organism known to us, the reply must be—a thinking

brain! But this brain, in order to communicate with the external world, requires organs of sensation, organs, too, by which it can react upon surrounding matter, and others by which it can remove itself from place to place. These processes, however, require again the expenditure of energy, which the brain has not the power to create out of nothing. Hence the necessity for what are sometimes known as the vegetative systems of man and animals,—the organs of digestion, assimilation, circulation, respiration, secretion, and excretion,—whose duty is to provide for the waste of tissue of the brain and its more immediate accessories, to place at their disposal a constant store of available energy, and to provide for the removal from the totality thus constructed of all matter which has become effete. Such a complex being obviously cannot be flung at random into space. It demands a world, without which there can be neither scope for its activity nor even the possibility of its existence. With this world it must be in harmony, and thus the animal becomes subject to the dominion of gravitation, with which its organic forces act now in conjunction and now in opposition. Here indeed, just as in the case of heat, we find that there are certain limits within which only it is possible for organic life as we know it to be maintained. Were the earth's attractive force as exerted upon bodies placed on its surface, either greater or less, we should all,—the "we" including every animal and vegetable organism as well as man,—in virtue of that difference alone, be something other than what we now are.

A couple of ideal experiments, the results of which no one can doubt, will make this clear, and will at the same time explain the part played by gravitation in the animal economy. Let us suppose this force doubled. All terrestrial animals would then be compelled to exert a vastly increased amount of strength in order to support themselves in any other than a recumbent position—to rise up from the ground, to run, leap, climb, or to drag or carry any object. In order to do all this several important changes in the animal structure would be necessary. Their muscles must necessarily be more powerful, and the skeleton to which they are attached would need a corresponding modification. To work such limbs a more rapid transformation of matter would be required; hence the supply of nutriment must be greater, involving enlarged digestive organs, and doubtless a larger respiratory apparatus, to allow of the perfect aëration of the increased mass of the blood. In order to keep up the circulation with the necessary force, either the heart would require to be more powerful

or the distance through which the blood would require to be impelled must be reduced, and the vessels widened so that there might be less loss from friction. The increased amount of nutriment required would involve a corresponding increase in the difficulty of its collection, and the struggle for existence would therefore probably be intensified.

These considerations involve a very marked change in the structure of land animals. Their general form, in accordance with their thickened bones, increased muscles, and larger respiratory and digestive apparatus, would be heavier and more massive than what we now observe. The necessity for such a structure would be increased by another circumstance, viz., that the probability of a fall, and the danger, if it should happen, would both be increased. Hence the bipedal form—whether among anthropoids, birds, or hopping lizards—would be attended with much greater drawbacks than is the case with the actual amount of gravitative force. Even quadrupeds would be unsafe in their movements, and it is highly probable that a hexapod, octopod, or decapod structure would be found extending much higher in the animal kingdom than it does in our world. Or if there is some reason why the number of limbs in a vertebrate animal cannot exceed four, we should find the majority of animals furnished with short legs, like those of Saurians, which raise the trunk of the body but little, and allow it to rest easily upon the surface of the ground. The type which we see in serpents would probably be much more common, and would be developed into a great variety of forms. The necessity of keeping the centre of gravity low, and the great demands made upon the system in other respects, would conspire to reduce the size of the head and the brain.

Of all animals those which pass a large proportion of their existence in the air, supported upon wings, would meet with the greatest difficulties. The earth would draw them down with a force hard to resist, and which could only be overcome by the expenditure of a large amount of muscular force, and consequently of the transformation of much matter. Hence with gravitation intensified, as we are supposing, such forms of life as the humming-birds, the swifts, the swallows, &c., could scarcely exist. In their stead would flourish groups approaching to the Gallinaceæ and the Struthionidæ, along with the true woodland birds, whose flight is little more than an extended leap from branch to branch.

Winged insects would fare very similarly to birds. The

difficulty of flight being greatly increased, we might look for a corresponding diminution in the number of aërial species, and an alteration in the habits of such as do exist. The dragonfly, which in order to find a sufficiency of food hawks about through the whole of the day; the tiger-beetle, who seizes his victims by a series of bounds, and short though rapid flights; the butterflies and bees, who have to visit thousands of flowers in order to obtain a sufficiency of honey or of pollen—must all find the struggle for existence rendered more arduous, and would doubtless be less numerous both in species and individuals. Hence the fertilisation of flowers by the intervention of insects must be impeded, and anemophilous species would receive an advantage which they do not now possess, and which would lead to the extinction—or at least to the great reduction—of entomophilous plants, *i.e.*, all those with the showiest blossoms: a somewhat unexpected result to follow from a mere increase of the earth's attraction! Creeping insects would merely find their position deteriorated in the same manner as would land animals. Hence they would have a relative advantage over their winged brethren, and might probably become in consequence more abundant. Such articulate forms as the centipedes, the scorpions, and the ground-spiders—having, by reason of their numerous legs, many points of support—would fare the best, though even such creatures would find it more difficult to run, and much more so to climb and leap, than they do at present. The ordinary cobweb-making spiders would have to make their nets of an increased strength, in order to support their own weight and that of their prey. The true gossamer-spiders, which now are able to mount up in the air to a considerable height, would find their flights at an end.

The terrestrial Mollusca—snails, slugs, and the like—would be little incommoded, so long at least as their journeys were confined to the level ground. Hence they would undoubtedly play a more important part.

Such, then, are the changes which would ensue, for instance, in our own globe, from a mere increase of its attractive force. Numbers of animal and vegetable species would find the balance so far turned against them that they would more or less rapidly succumb. Other species would find increased scope and opening, and, if the Evolutionist view be correct, would doubtless branch out into a multitude of new forms. But all, to survive, would have to be structurally modified to a greater or less degree in the manner I have already pointed out.

Did a man weigh three times as much as he now does—as would be the case were he placed on the surface of Jupiter—the conditions of organic life must be still further altered, and the resulting forms, other conditions remaining equal, would differ still more widely from what we see around us.

Decreased attraction of the earth, or a reduction in the actual weight of plants and animals, would be attended with another set of changes scarcely less remarkable. With the same expenditure of vital energy as at present, and with the same quantity of transformation of matter, we might be able to lift larger bulks, to take longer bounds, to move with greater swiftness, and to undergo prolonged muscular exertion with less fatigue. Hence the transformation of matter required to keep up animal heat, and to restore the waste of tissue, would be smaller for the same amount of duty done. A less volume of blood, smaller lungs and digestive organs would be required. The labour of the heart, in raising the blood to the head and parts of the body above its own level, would be reduced. Hence we might expect a set of structural changes in terrestrial animals of an inverse nature to those we have seen resulting from intensified gravitation. All parts of the body might safely be constructed upon a less massive plan. A slighter skeleton, smaller muscles, and a slenderer trunk would suffice. These modifications would be the more essential from another consideration. At present the cases in which men or other terrestrial animals, are swept away by the wind are exceptional, and occur only in furious cyclones. Were our weight, however, reduced to one-fourth of its present magnitude, whilst our bulk remained unchanged, we should not seldom run the risk of being carried off our feet, with the possibility of unpleasant consequences. Hence one of the characteristics of the "fittest" for survival in such a state of things would be either a minimisation of the surface exposed to the wind, or the utilisation of atmospheric currents as a means of locomotion. On the water this is already done by the paper nautilus, which drives before the wind, using its arms as a kind of sails. On land we may see that most noxious Dipteron the "Harry long-legs" (*Tipula oleracea*) travelling in a brisk breeze, with a kind of motion that is neither running nor flying, but a curious mixture of both. Head-first or tail-first, rolling over and over, it makes a very rapid progress, as we find if we attempt to "stamp it out," though all the while its movements appear exceedingly clumsy. Were the force of gravitation reduced in the manner we are

contemplating, locomotion of this character would probably become common among the higher animals. The separation between the aërial and the terrestrial forms of life would be less marked than we now find. A true aërial life, moreover, would be much easier than at present, as the winged animal, of what kind soever, would require to exert much less force than at present in order to overcome the attraction of the earth. Hence the number, the variety, and probably the size of winged species would doubtless be much increased.

We may safely assume that could gravitation be sufficiently intensified we should ultimately reach a maximum limit beyond which life could not exist. A point is easily conceivable where no force generated within the body of an animal—supposing one placed in such circumstances—would counterbalance the force of which it was, so to speak, *cemented* to the earth's surface. All motion from place to place would then be absolutely impossible, and every movement within the animal system, save in a line tending to the earth's centre, would be impossible likewise. The smallest and simplest animals would doubtless bear a greater gravitative force than the higher and larger. It is evident that they have, even now, a certain advantage in this respect. Though under the influence of gravitation, as decidedly as are larger species, we see them—even when not specially organised for such feats—ascend smooth perpendicular surfaces, and even travel along the lower side of a horizontal plane with their heads downwards. Naturalists have spent much time in explaining this faculty in the common house-fly, which, however, is only one out of thousands of species able to perform the same action.

A fall, even from a height very great in proportion to their stature, seems to have no injurious effect upon wingless insects. This may at first sight seem due to the coat of armour, or so-called exo-skeleton, in which they are enclosed. But if a tortoise is dropped from a corresponding height its armour does not save it from damage. The reason must, then, be sought in the resistance offered by the cohesive power of the atmosphere to the fall of so light a substance.

Even a relatively moderate increase in the force of gravitation might be productive of serious consequences to larger animals. In man, the downward pressure of the blood resulting from gravitation sometimes distends the veins of the feet and legs so as to produce a varicose condition. Were the human stature or the earth's attraction doubled, and still more if both these modifications took place con-

jointly, this disease would be induced earlier in life, and would occur not merely in the lower extremities, but in the regions of the trunk below the level of the heart. The increased action of the latter organ, further, could scarcely fail to bring on aneurism.

If we then admit that gravitation tends to limit the size of organic beings, presenting difficulties which grow with their growth, we shall be in a position to explain certain general phenomena of the animal and vegetable kingdoms which have hitherto been for the most part accepted as ultimate facts. It is a familiar observation that every substance, living or lifeless, weighs less the denser the medium in which it is plunged, or, more accurately speaking, the nearer the specific gravity of such medium approaches its own. If we raise a stone which is lying under water, we find its weight increase suddenly the moment we have lifted it above the surface. This simple experiment may show us that a creature inhabiting the land, and having its body ordinarily immersed in air, experiences greater difficulty in supporting its own weight than would a being of equal size and formed of similar materials, but framed to live in the water. The former has to spend a greater proportion of its energies in fighting against gravitation than the latter. Again, a creature which passes a great portion of its time not merely immersed in air, but suspended in it, has a still harder struggle against gravitation than either of the two former, and will find its vital force still more severely taxed. Hence we might expect to find the inhabitants of the air, in the strictest sense of the term (*i.e.*, *Cheiroptera*, birds, and winged insects), small; the inhabitants of the water largest; whilst the dwellers on land—immersed, indeed, in air, but supported to a great extent by the firm ground—should occupy an intermediate position.

This agrees substantially with what we find on an actual examination of the animal kingdom. If we take the two leading sub-kingdoms, the Annulosa and the Vertebrata, each of which includes a vast number of aquatic, of terrestrial, and of aërial species, we find that in every case their largest-sized members are inhabitants of the waters. The first of these sub-kingdoms includes the insects, the myriapods, the spiders and their allies, and the crustaceans. But everyone knows that certain of the latter group, such as the crabs and lobsters, far surpass the remaining classes in magnitude. Who, for instance, ever found a beetle, a cockroach, locust, scorpion, centipede, or spider weighing 5 lbs.? Yet a crustacean of 15 to 20 lbs. is by no means unexampled.

Turning to the sub-kingdom of the Vertebrates, popularly so much better known, we find in its highest class, the Mammalia, the largest of all known animals, the whales, all strictly aquatic. Several of the seals—such as the bottle-nose, which reaches a length of 25 feet—and the walrus also far exceed the average stature of land-animals. Nay, amongst those ordinarily classed as true terrestrial forms, many of the largest—such as the hippopotamus, the elephants, the various species of rhinoceros, even the true buffalo (*Bubalus bubalis*) are marsh-dwellers, semi-aquatic in their haunts and habits. The largest of the cats, the Bengal tiger, is also the most natatorial.

Among the reptiles we find a state of things closely analogous. Of the Saurians at present living no terrestrial species can at all compare with the crocodiles. Among the gigantic extinct groups the majority at least—such as the Ichthyosauri, the Plesiosauri, and the Pythonomorphæ (if the latter are not rather Ophidian)—must have been aquatic.

If we examine the serpents we find, again, that the largest species are aquatic,—as, for instance, *Eunectes murinus*,—inhabiting the rivers of South America.

Among the Chelonians the same rule prevails, the turtles being certainly much larger than their terrestrial kindred, the tortoises, even those of the Gallapagos and the Mascarenes.

Of the Mollusca only a minority are terrestrial. But here also we find the smaller species inhabiting the land, and the larger forms, especially the gigantic cuttlefishes, confined to the waters.*

I may here remark that Swainson, though setting out from totally different considerations, and though accepting each animal species as an independent ultimate fact, lays a decided emphasis on the magnitude of "natatorial or aquatic types," which he pronounces "as to structure, chiefly remarkable for their enormous bulk."†

In opposition to the vast growth of the aquatic groups, we find the inhabitants of the air for the most part diminutive. The bulkiest birds are puny in comparison with the bulkier members of the other vertebrate sections. Further, even among birds, the largest species—such as the ostrich, emeu, and cassowary—are strictly terrestrial in their habits, as was also the gigantic extinct moa (*Dinornis*) of New Zealand, and in all probability the still larger *Æpiornis* of

* Cephalapods are known to reach the weight of 2 tons.

† Geography and Classification of Animals, p. 249, § 309.

Madagascar. By way of contrast we may point to the humming-birds, perhaps the most truly aërial of vertebrates, some of which do not exceed 20 grains in weight, and which, except when sleeping or engaged in the act of incubation, are ever on the wing.

The largest and heaviest insects,—such as *Goliathus Drurei*, *Dynastes elephas*, &c.,—though possessing organs of flight, use them rarely and clumsily, and may in this respect be instinctively contrasted with many species of tiny Diptera and Hymenoptera which spend nearly the whole of their adult stage in the air.

An examination of the respective sizes of the various animal groups fully supports, therefore, the assumption with which I started. There are, indeed, multitudes of tiny and even microscopic creatures inhabiting both the salt and the fresh waters. But the largest species, whether still living or extinct, are strictly aquatic.

The vegetable kingdom in its turn presents us with very similar phenomena. The dry land, indeed, produces mighty plants like the Sequoia of California, the Boabab of Senegal, and the Jarra-jarra and the gum-trees of Australia. But according to Prof. Reinsch, the *Macrocystis pyrifera*, a sea-plant found in the North Pacific, surpasses them all. A single specimen of this plant has been found, by actual measurement, to cover three square miles.

The size of extinct organisms, as compared with that of species still surviving, has often attracted attention. Here there certainly exists, in the popular views, a very considerable amount of exaggeration. Ever since it was placed beyond doubt that fossils were truly the remains of plants and animals which once inhabited this earth of ours, and not mere *lusus naturæ*, or superhuman forgeries—we can find no better name—created specially for man's delusion, it has been customary to invest the faunæ of bygone geological epochs with a magnitude most portentous. But, as is the case with recent forms of life, this gigantic character shrinks when the test of careful measurement is applied. We may safely say that no fossil serpent, saurian, fish, or whale of one hundred yards in length has yet been discovered; that no strata have yet disclosed the remains of a terrestrial carnivore of double the size of the royal tiger, or of a proboscidean, ruminant, edentate, or marsupial of more than thirty feet from head to tail. It has even been questioned whether the mean stature of the animal kingdom has greatly decreased. Still we have good evidence for concluding that in almost every group species have once existed

very considerably surpassing in size their nearest representatives still living, and that those orders which now comprise our bulkiest animals, such as the proboscidea and ungulata, were at one time far richer in species than at the present day.

The relative magnitudes of the various animal groups do not seem to have been greatly different from what we now find them. Early geologic epochs certainly display birds larger than any now living. But the largest of these birds did not equal, much less exceed in stature, the large mammalian forms of those days. The *Harpagornis* was smaller than the *Machairodon*, just as the harpy eagle of our time is smaller than the tiger or the jaguar. The mammoth and the mastodon exceeded in bulk the *Dinornis* and *Æpiornis*, just as the elephant surpasses the ostrich. The rocks have yielded up remains of insects larger, perhaps, than insects of the same, or of closely approximating orders existing in the present day. But we find no proof that the *Annulosa*, as a class, made any nearer approach to the average growth of the *Vertebrata* than what we observe in living species. This fact, we submit, powerfully supports the view that the relative as well as the positive sizes of organic beings are not solely dependent upon such circumstances as the greater or less abundance of food, the competition of other species, &c., but are governed by some deeper law. Evolution has no *tabula rasa* upon which to work. We do not, of course, ignore or question the fact that the respective parts played by certain of the leading groups of the animal kingdom have not always been the same; that there has, for instance, been an epoch in the earth's existence which might be pre-eminently considered as the age of reptiles, when the development of the mammalia was only in its infancy.

That in so many cases the largest, strongest, and best-armed species should disappear, whilst the smaller and the weaker have been perpetuated, is a curious case of the survival of the fittest. This superiority demands an explanation. If, as seems not improbable, the density of the atmosphere was greater in the early geological epochs than is now the case, we have a clue to the more luxuriant growth of the terrestrial and aërial species of those days. They would then be placed in conditions somewhat approaching those of the aquatic animals, and might accordingly attain a larger size.

It appears, therefore, that although gravitation sets cer-

tain boundaries to the size of organisms which they are unable to exceed, yet these boundaries vary with the density of the medium in which such organisms are immersed.

A denser atmosphere, just like a reduced gravitative force, would tend to obliterate the sharp boundary line now existing between terrestrial and aërial animals, and would doubtless re-people the world with winged saurians, gigantic birds, and with flying mammals.

The effects of a highly rarefied atmosphere may easily be realised from what is witnessed upon lofty mountains. Great limitation among aërial species, and an equally great debilitation in terrestrial forms, could not fail to result.

There has been no little speculation as to how the world must appear to beings whose whole life-time might be counted by minutes rather than by years. But no one seems to have entertained the idea that our perceptions of external nature and our modes of interpreting phenomena are to a very great extent the result of our size, and would change if it were modified. Such a contemplation may teach us some useful lessons.

Let us in the first place suppose ourselves reduced to the size of the minutest living being, though preserving the same mental faculties, and, in proportion to our bulk, the same physical powers which we now enjoy. In what light would the most ordinary phenomena of outward nature present themselves? We should find an alteration, not merely in magnitude, but in kind. As an example, I set out for a journey of exploration upon a cabbage leaf, which appears as a plain of many square miles in extent. I find it studded with huge, glittering, transparent globes, each in height vastly exceeding my own stature, and resting motionless upon the surface of the leaf. Each of these appears to pour out, from one of its sides, a dazzling light, accompanied by a strong heat. Urged by curiosity, I approach and touch one of these orbs, when suddenly I feel myself seized upon and whirled round and round till I come somewhere to an equilibrium and remain suspended on its surface, utterly unable to extricate myself. In the course of an hour or two I find the globe diminishing, and ultimately it disappears, leaving me at liberty to pursue my travels. Leaving the cabbage leaf, I stray along over the surface of the soil, which has an exceedingly rocky and rugged character, until I see before me a broad surface of the same kind of matter which formed the globes on the cabbage leaf. Instead, however, of rising upwards from its support, I see it now sloping downwards in a vast curve from the brink, and

ultimately becoming apparently level, though as this is at a considerable distance from the shore, I cannot be absolutely certain. Let us now suppose that I hold in my hand a vessel bearing the same proportion to my minimised frame that a pint measure does to that of man as really existing, and that by adroit manipulation I contrive to fill it with water. If I invert the vessel, I find that the liquid will not flow out, and can only be dislodged by violent shocks, or by the removal of the atmospheric pressure from a part of its surface. After these and a few more observations and experiments easily conceivable, I sit down to theorise on the properties of water and of liquids in general. Shall I come to the conclusion that liquids seek their own level; that their surfaces when at rest are horizontal, and that solids placed in a liquid sink or float according to their higher or lower specific gravity? No; I shall think myself justified in inferring that liquids, when at rest, tend to assume spherical, or at least curvilinear forms, whether convex or concave, depending upon circumstances not easily ascertained; that they cannot be poured from one vessel into another, but, unlike solids, resist the law of gravitation, which is consequently not universal, and that such bodies as I can manipulate refuse to sink into liquids, whether their specific gravity be high or low. From the behaviour of a substance placed in contact with the dewdrop I shall even derive plausible reasons for doubting the inertia of matter. All these changes in my interpretation of phenomena arise not from my becoming aware of any forces hitherto overlooked, still less from the disappearance of laws now recognised, but simply from the fact that my supposed decrease in bodily size brings capillarity and cohesion into a relative prominence which they do not now possess. To rational beings of the actual size of man the effects of capillary attraction and of the cohesion of liquids rank among the residual phenomena which only attract attention when science has made some progress. To *homunculi* such as we have been supposing the same effects would be of capital importance, and would be interpreted not as something supplementary to those of general gravitation, but as due to an independent and possibly antagonistic force.

Their vision might possibly surpass, in its power of dealing with minute objects, even our most exquisitely finished microscopes. But this faculty, however invaluable to beings who could use it only when and where they might think desirable, would be rather bewildering than enlightening were it the only mode of vision. To see objects at will with

say a power of one-twentieth inch is exceedingly useful; to see them with such a power only would show merely details which the mind could scarcely combine into an accordant whole.

In the study of heat they would encounter difficulties probably insuperable. In this branch of physical investigation little can be done unless we have the power of raising and lowering the temperature of bodies at pleasure. This requires the command of fire. Actual man, even in a very rudimentary state of civilisation, can heat and ignite certain kinds of matter by friction, percussion, concentrating the sun's rays, &c. But before these operations can produce actual fire they must be performed upon a considerable mass of matter, as otherwise the elevated temperature is conducted or radiated away as rapidly as produced, and the point of ignition is never reached. The physics of such *homunculi* would therefore differ most remarkably from our own.

Nor could it be otherwise with their chemistry, if indeed such a science can be conceived as possible for them at all. It can scarcely be denied that the fundamental phenomena which first led mankind into chemical inquiries are those of combustion. But, as we have just seen, such beings would be unable to produce fire at will, and would have little opportunity for examining into its nature. They might occasionally witness forest-fires, volcanic eruptions, &c. But such grand and catastrophic phenomena, though serving to reveal to our supposed tiny men the existence of combustion, would be ill-suited for quiet investigation into its conditions and products.

Let us pass now from Lilliput to Brobdignag, and consider how nature would appear to rational beings of enormous magnitude. Their difficulties and misconstructions would be of an opposite nature from those committed by the pygmies above imagined. Capillary attraction and the cohesion of liquids, instead of being over-estimated, would be doubtless entirely ignored. The dew-drop and the curvature of collections of liquid, where bounded by some solid body, would be altogether invisible. The behaviour of minute bodies thrown upon a globule of water would escape notice. The *homuncnlus* able to communicate but a very small momentum will find all objects much harder than they appear to us, whilst to the colossal beings we are now supposing granite rocks would be a very feeble impediment.

But the most remarkable difference between such enor-

mous beings and ourselves lies here: if we stoop down and take up a pinch of earth between the fingers and thumb, moving these members say through the space of four inches in a second of time, we experience nothing remarkable. The earth offers a little resistance, more or less, according to its greater or less tenacity, but no other perceptible reaction follows.

Now let us suppose the same action performed by a gigantic being, able to move his finger and thumb through four miles of soil in the same lapse of time. He would experience a very decided reaction. The mass of sand, earth, stones, or the like, hurled together in such quantities and at such a speed, would become intensely hot, probably reaching the point of ignition. If his size were still greater, the experiment would be attended with still more striking results. If we can suppose a being vast enough to grasp the earth between his two hands and at once arrest its motion, the consequence would be explosive ignition capable of resolving at once the planet, and the being who thus checked its career into a gaseous condition. Just as the *homunculus* must fail to bring about ignition when he desired it, so the colossus could scarcely move without causing the liberation of a very inconvenient degree of heat, and literally making everything too hot to hold. He would naturally ascribe to granite rocks and the other constituents of the earth's surface such properties as we attribute to phosphorus—of ignition on being a little roughly handled.

Need we do more than very briefly point to the obvious moral? If mere differences of size can cause some of the most simple facts in chemistry and physics to take so widely different a guise; if beings infinitesimally small and immensely large would simply as such be subject to the delusions we have pointed out, and to others that might be detected, is it not possible that we, in turn, though occupying, as it seems to us, the golden mean, may also in the mere virtue of our size fall into misrepresentations of phenomena from which we should escape were we either larger or smaller? May not our knowledge have in it in this respect an element of subjectivity hitherto unsuspected, and we fear scarcely possible to eliminate?

III. SANITARY SCIENCE IN THE UNITED STATES: ITS PRESENT AND ITS FUTURE.

By Albert R. Leeds, Ph.D.

IT is generally conceded, I believe, by scientific labourers in this country that we have been more fertile in invention than discovery. We owe to older nations a constantly increasing debt of obligation for those initial germs of thought which have fructified into new sciences, while we may, at the same time, ask a generous acknowledgment of the merits of many inventions which have opened up new fields of employment to thousands. Sciences which promise much for the improvement of the daily condition of mankind, and have in them a side largely practical, are sure of welcome in our midst. Such a science is pre-eminently the one under consideration. It gathers into one the teachings of all other sciences, so far as they bear upon private and public health, and makes these teachings practically operative in the promotion of human welfare in this country. It grew into prominence during the war of the Rebellion, when the work of the Sanitary Commission was made co-extensive with every army camp and army hospital. Its principles have been expounded in sanitary associations formed in many States and in smaller communities. These have led to the formation of State and city boards of health, clothed to a greater or less degree with executive functions.

Every epidemic has fastened popular attention upon the subject, and before it was taught in book or lecture-room, has been rehearsed in a thousand forms in the newspaper. In this present yellow fever plague more than twelve thousand people have perished, probably not less than sixty thousand have convalesced, and two hundred millions of dollars would not represent the aggregate pecuniary loss. During its awful course universal interest has been felt in the cause and prevention of this and similar diseases, a homily on private or public hygiene has formed a prominent feature of the daily paper, and this interest has culminated in the offer made by a lady, already widely known by her munificence in the cause of science, to defray the expenses of a commission of enquiry composed of sanitary experts. We believe that this is all as it should be, and that in sanitary science this country is taking a foremost place, because

popular sympathy and popular knowledge are running almost abreast of the science itself. The proper execution of sanitary laws demands the free and intelligent co-operation of the individuals; a system of enactments, however skilfully framed and however supported by a strong central authority, alone will not suffice. Not only would it appear alien to the genius of our institutions, but also a mode ill-suited to attain its object, if a Health Department were added to the other departments of State at Washington. No one would deem it possible for such a department to legislate pure air, pure water, and pure food into use throughout the nation. On the contrary, it would appear far wiser to leave such legislation to each State, and in the State as far as possible to each community, recognising that the popular agitation and knowledge requisite to obtain health laws is the best guarantee that they shall not afterwards remain dead letters. What I have to say concerning the present and future of sanitary science is mostly the record of what such communities have already done, and of what ideas are already growing up in their midst, and which ere long will bear fruition in new laws and movements.

I. *Vital Statistics.*

We shall begin at the foundation-stones of exact sanitary science—vital statistics. To appreciate the advance in this direction we have only to compare the condition of our great city, New York, at the beginning of this century with its present. At that time it had no registration even of deaths. The first "Bill of Mortality," as it was called, extended from November 1, 1801, to January 1, 1803—fourteen months. So little accuracy in the nomenclature of diseases was thought of or expected, that in this report the people are said to have died of "flux," "hives," "putrid fever," "rash," "lingering illness" (which certainly was not a rash performance), "stoppage," "breaking out," "fits," &c. The first reliable report was that made in the year 1866, after the organisation of the Metropolitan Board of Health. In the second annual report in 1867, the beneficent results of the institution of the Health Board, and of the sanitary reforms executed under its superintendence, were shown by the fact that there were 3152 less lives lost during the first year of its administration as compared with the preceding year. The report, moreover, showed that this gain had been mainly in the chances of life at the adult ages, and in the districts where the greatest amount of sanitary work

had been performed. The causes of insalubrity affecting infant mortality were not yet within control. In the year 1868 the work of registration was extended and specialised in such a way that comparisons could be made in the death-rate between portions of the city occupied by a degraded and overcrowded population and those more favourably situated, whether in point of natural advantages or in the character of inhabitants. This specialisation enabled the sanitary inspectors to judge of the value of their labours in the matter of cleansing and disinfection. In fact they had lowered the death-rate in certain of the most wretched wards below that in some of the best, sanitation in these latter having been omitted. The registration extended also to the effect of modes of living upon the death-rate, and in this manner pointed out the necessity of controlling the excessive mortality in tenement houses. That health reports, when promptly and intelligently used, might be effectively employed in the prevention of disease, was shown by the returns made during the last two weeks in July, 1878. The Registrar, apprehending that the infective quality of Asiatic cholera might prove to be present in the rapidly fatal diarrhœas then prevalent, sent warning to the Surgeon-General of the United States army, in consequence of which the General in charge of the recruiting and transportation of troops ordered the immediate suspension of that branch of the army service in New York. Valuable illustrations of the relation existing between damp houses and pulmonary consumption were obtained by selecting certain wards of the city and forming maps in which every death from phthisis for a number of years was marked on the chart opposite the locality of its occurrence. The evidence so obtained pointed to an excess of consumption at the lowest levels, and in two of the wards to a crowding of fatal cases of this disease in localities unusually damp and in rainy seasons flooded, although these dwellings differed in no other respect from the average of the ward. The results obtained in this manner were deemed so valuable that the registration was applied to each house in the city. In this way excessive mortality in any locality or from any special class of diseases became known at once to the sanitary inspectors.

With regard to the registration of marriages, improvement was more difficult. The system of registration, expecting a voluntary support from clergymen and civic officers concerned, could secure very partial returns, and it was only by diffusing information through the press and the lavish distribution of circulars that the accuracy and com-

pleteness of the returns in this respect could be improved. An enquiry into the number of births registered, as compared with that which the Board had reason to believe occurred, revealed a deficiency in the registry of 65 per cent. It has been stated by an American writer on these topics that "it would be impossible for a large portion of the adult men and women born in the United States to prove by any public records or other legal documents that they were legitimate offspring, with a natural right to the name they bear, or even that their parents were ever married."

The system of mortality registration was gradually improved until the returns made in the year 1871 were probably nearly perfect. When compared with the mortality in other cities, this accuracy, however, told against New York, for while its death-rate was 28·6 per thousand, that of St. Louis in the same year was reported at 17 in a thousand; of Rochester, 16; Buffalo, 14; and Jersey City, only 7. In matter of marriages and deaths, the increased knowledge among clergymen, physicians, and others on whose voluntary co-operation the registration largely depended, had resulted in an apparent increase in the annual marriage and birth-rate, but still the number of births returned was probably less by 10,000 than the true. In the following year the Board instituted suits against these parties, which had a beneficial effect, but it became evident that nothing short of important changes in the law would secure completeness.

I have been thus particular in narrating the history of vital registration in New York because this city was the first to undertake a reform, and because its reports were the first which attempted to keep abreast with the development of sanitary science and to diffuse this knowledge broadcast. The course of legislation on these points is one which every city and State has, or is going through. In reference to New Jersey, the facts are so fresh that I scarcely need recall them. At each meeting of the New Jersey State Sanitary Association since its origin, three years ago, the inaccuracy and worthlessness of the State vital statistics were conclusively shown in the reports of the committees on this subject. The Association formulated a protest, and appointed a special committee to memorialise the Legislature. By these means, and by the efforts of the State Board of Health, public opinion on the subject was awakened and so far educated that during the winter just passed a law was enacted which gives to New Jersey one of the best systems of registration as yet devised in this country. It has incorporated in it two features to which its peculiar

excellence are due, and which should be universally copied: 1st. That of issuing burial permits only after registry has been made by a properly qualified person; 2nd. The returns are made to an expert, who collates them in accordance with the views of the most eminent authorities, and draws from them their most important teachings of immediate and very practical application.

II. *Registration of Disease.*

We must not rest content, however, with the returns of mortality; we should advance to the registration of disease. This is practicable, and if not in all, yet in that large class of diseases in their nature preventable, of universally acknowledged utility. We do not delay until a street brawl becomes a riot before notifying the magistrate and securing police aid; neither should we wait until diphtheria, typhoid, &c., become epidemic before sending intelligence to the custodians of the public health. But this is not all; to make their knowledge of public utility, these custodians must be invested with adequate powers. At present there is little more expended upon the whole work of the Board of Health of the State of New Jersey during an entire year than the pay of two policemen. Its members labour without remuneration for the *sanitas publica*. Their power is mainly the educational impetus of just ideas, forcibly expressed. There are many ways of promoting sanitary reforms, but none, it appears to me, so practical as that of giving to the Health Board the money, means, and men to register diseases, to investigate their causes, to suggest and promote their remedies, and not unfrequently to bring offenders to suitable punishment.

III. *State Sanitary Legislation.*

There is a source of danger, as this last summer has strikingly shown, which cannot be warded off by sanitary legislation when limited to a few of the States. If those States which are the seats of yellow fever year after year do not provide efficient precautions to suppress or control the epidemic, it will annually invade other localities, following the lines of travel, and spreading northward in the Mississippi basin. We have recently seen the alacrity with which more favoured communities came to the relief of those afflicted with the epidemic. Help of every description was sent until the bountiful public was asked to hold its hand.

While the terrible plague lasted was not thought a time for good advice, but for good deeds. Now that the danger is over, the time has arrived to avert similar visitations in future. Does it appear unreasonable to ask for the most skilfully-devised sanitary regulations in localities where such a pestilence may germinate? Recent events have elicited a vast deal of discussion as to the origin of these epidemics and the modes of combating them. There is want of harmony, however, in all points but this: that some of the factors which are concerned in originating the disease are within human control, and prevention, therefore, is the duty of the authorities where the disease germinates.

Those involved in the consequences of neglect of these duties, however remote their homes, have a right to ask for reform. This agitation should not be allowed to die out with the pressure of the calamity which aroused it. It should be continued until every one of the States has an efficient health code. At present the majority have none, or very deficient health laws. Massachusetts has strikingly shown its general enlightenment by being the first State to have an efficient Health Board and a wisely-devised code of sanitary legislation. New York and Pennsylvania have neither, though strenuous efforts have been made by public-spirited individuals to do away with the stigma. In the west, Michigan has been distinguished by the excellence of its sanitary legislation, and the voluminous and valuable publications of its State Health Board. But Arkansas and Missouri are sadly deficient, and the case is even worse in Iowa, Kentucky, and Indiana. Some attempts to supply the most pressing wants have been made in Florida and in North and South Carolina, and health laws are not entirely wanting in the statute-books of New Hampshire, Maine, and Rhode Island. The necessity for educating the people in each State before the requisite legislation is secured, will require a considerable period to elapse before all the States have systems of laws in accordance with modern knowledge. In the meantime, in the name of all those good men who have perished, and as an acknowledgment of the nation's charity, let the plague-stricken States of the Gulf and the Mississippi basin lose not a day in adopting the wisest precautions experience and investigation can offer.

Struggling, as we are in this country, to have the importance of sanitary legislation generally recognised, the progress made in some directions is highly encouraging. It is probable that no community will take steps to learn what is essential to its health before it has suffered from lack of

information. To the distress of London the world owes those great works of the Royal Commissions on Water Supply and the Pollution of Rivers, which are the repertory of the best knowledge on these topics. The manufactories of England have made it necessary for the Government to take cognisance of aërial impurities, and much has been done in that country towards establishing a chemical climatology. Similarly the pollution of the Passaic by the manufacturing towns above has caused enquires to be set on foot akin to those referring to the pollution of the Thames, and has given rise to extended enquiries into the methods and aims of water analysis.* An attempt was made to deprive the inhabitants of New York of some of their public parks and occupy them with buildings devoted to military and other purposes. The more public-spirited citizens came to the rescue, and, through the influence exerted, a Public Parks Association and other means preserved the open squares as breathing places and pleasure grounds. The Association recognised as its principle of action that to preserve the parks they must be improved. The proposition was made and eloquently advocated by Dr. Seguin that the physical as well as the spiritual well-being of the citizens at large would be powerfully augmented by making the public gardens out-door schools, supplementing the in-door school system by that in which they are lamentably deficient—an education in the phenomena of plant and animal life. A beginning in this direction has been made in the Botanical and Zoological Museums of the Central Park of New York, and in the Fairmount Park in Philadelphia; but these are remote from the centres of population, and the objects of study should be placed where they could constantly appeal to the eye. The hygienic value of gratifying the sense of beauty, as well as satisfying the requirements of use, is more and more recognised. The first society on this side of the water organised with this object was the so-called Laurel Hill Association, of the village of Stockbridge, in Western Massachusetts. After twenty-years of activity, the result has been to produce a village of exceeding loveliness. Thousands of trees have been planted out along the road-

* "Report to the Board of Public Works of Jersey City," Profs. WURTZ and LEEDS.

"Analytische Beiträge aus dem Laboratori un l des Stevens Institute of Technology, Prof. LEEDS: Zeitschr. für Anal. Chem., 1878.

"Recent Progress in Sanitary Science," Prof. LEEDS: Annals of the Academy of Science, New York, vol. xi., 1878.

"Water Supply of the State of New Jersey," Prof. LEEDS: Journ. Franklin Institute, March and April, 1878.

sides. The village cemetery, formerly neglected, has been surrounded by an exquisitely-kept hedge. Monuments have been erected to the memory of villagers whose subsequent achievements have made the place of their birth illustrious. Prizes have been offered for those who laboured most efficiently to improve the health and beauty of their native town, and for these prizes the poor as well as the opulent contend. In fact, the neatly-kept sidewalks, the attractive gardens, the pretty cottages of the poor, are a better indication of what healthy village pride can do for a community than the tunic lawns of the rich. I need not add that in a community where these things which add grace and beauty to the daily life have been done, the more important works of water supply, drainage, sewerage, &c., have not been left undone. Similar associations have sprung up throughout New England. In Williamstown the villagers have thrown down every fence, and this most picturesque of village towns is a beautiful park, through which the houses of the inhabitants are scattered.

IV. *Ventilation.*

In the matter of ventilation, a considerable advance on the whole is to be noted; in other words, the percentage of failures to successes, in cases where methods of ventilation for the time being in vogue have been tried, is slowly growing smaller. The volume of scientific literature, founded on our increasing knowledge of the properties of materials, of gases, and of heat, grows more rapidly than the generally accepted rules by which the art of ventilation is to be practised. It is noteworthy that there are few persons who do not regard themselves competent to arrange the ventilation of an ordinary building, and it has hitherto been left largely to the builder, the vestryman, and the school trustee. This should not be the case. What advance has been made is mainly due to the specialisation of this kind of professional labour—the foundation of a class of engineers who devote themselves exclusively to problems of this character, and who have fought their way into practice by successful work accomplished. The architect submits his plans to these specialists, who adds to them the requisite details of heating and ventilation. It would be a great step in the interests of sanitary science if the school or hospital trustee would not think it devolved upon them, as a portion of their office, to become for the time being an authority upon ventilation, and if they were, as a proceeding of sound economy, to rele-

gate this duty to persons properly qualified. As a matter of fact, these qualifications are obtained at a considerable expense to the commuity, for in this stage of the art of ventilation, there being no authority universally recognised and but few generally-conceded rules, every sanitary engineer goes through a somewhat similar series of experiments and failures before he arrives at a reasonably successful method in practice.

As far as I can learn, there appear to have been great successes as well as great failures, whether the system of ventilation by aspiration has been resorted to, or that by propulsion. At the present time many authorities of note have declared in favour of mechanical ventilation. And yet in a number, I might say in most of the asylums and hospitals in this country where fans have been introduced, they are now standing still. The Roosevelt Hospital, for instance, in New York, where the fan, after having been put in operation, was run backward, and run so for months. It is now stopped. This is one fact of many which would make us chary of affirming positively that either system is the better. Probably both, discreetly applied, yield good results, and in their skilful application, and not less the faithful supervision of the ventilating apparatus after introduction, are good results to be sought.

V. *Physical Education in Schools and Colleges.*

Progress in this direction has been initiated at our highest seminaries of learning, and is slowly working its way downward through our educational system. I do not refer to so-called athletic sports, although these had not attained to much prominence in our colleges prior to the year 1850, but to the introduction of physical exercise and instruction on hygiene as a part of the college curriculum. This, so far as I am aware, was successfully accomplished in Amherst College; and now, after a trial of nearly twenty years, is still regarded as an indispensable adjunct of the college course. The dignity of this department of instruction is emphatically recognised by appointing to it only distinguished members of the medical profession, and including them in the college faculty on the same footing as the other professors. It is made their first duty to know the physical condition of every student, and to see that the laws of health are observed by them. In case of sickness the students apply to this officer for a suitable certificate, which excuses them from college duties, and are put in the way of

obtaining suitable treatment. The statistics of the bodily condition of the students are regularly and frequently secured, and are supplying a collection of "physiological constants," which are of growing interest, and supply practical helps in determining whether physical condition is within the bounds of health, and whether their development from time to time is normal or otherwise.

All the classes are required to attend gymnasium exercises four times a week, and the regularity and faithfulness in this is made an element of collegiate standing. The performances are accompanied with music, and arranged to give full play to the animal spirits. This and the advantages personally experienced by the students have conspired to make the gymnastic fully as popular and well-attended as the literary exercises. Finally, the intelligent co-operation of the student is secured by instruction upon the means of preserving health, physical and mental, with supplementary lectures upon human anatomy and physiology. Prof. Hitchcock,* writing of the chances of life of the young men under this hygienic discipline as compared with men at the same age elsewhere, says it is regarded as an established law that the chances of life grow less and less from about the fifteenth to the twenty-third year, and the rate of decrease is very rapid. But the tables of health, as kept at Amherst College, show that there is an improvement in health from year to year through the course, the ages being from nineteen to twenty-three. For taking the number of sick men in the freshmen class as unity, the number in the sophomore year as 0·912, in the junior 0·759, and in the senior but 0·578, the percentage of sickness during the college course diminished to nearly one-half.

In the light of this successful experiment, continued for a period of twenty years, it is not premature to urge upon colleges generally the formation of a similar department, and for my own part I see no method of raising the character of public school instruction so effectual as that of giving to the physical training of the children a very prominent place.

VI. *Health Resorts.*

The growing facilities each year for travel are steadily increasing the number of citizens who visit the country, the sea-shore, and the mountains. A salubrious village is fre-

* Hygiene at Amherst College, Prof. HITCHCOCK: American Public Health Association.

quently transformed into a centre of pestilence merely by such influx of strangers, the entirely natural modes of disposal of refuse and excreta no longer being adequate, and the artificial methods not being provided until an outcry due to disease is raised. In no way, however, is the growing intelligence on sanitary matters more strikingly shown than by the extreme sensitiveness of pleasure-seekers to the salubrity of summer resorts in respect to water, sewerage, and drainage. Of the multitude of illustrations I need but speak of Bethlehem, in New Hampshire, a beautiful village situated 1700 feet above sea-level, and so renowned for the purity of its atmosphere that of the 40,000 hay-fever patients whom Dr. Beard has calculated exist (and hay-fever patients say that life to them is only a tolerable existence) in this country, several thousands annually spend part of the summer there. Its popularity increased in a few years so rapidly that a crowded village soon arose, and during the summer of 1877 an outcry was made concerning drainage. The towns-people, realising that the reputation of salubrity was the wealth of the town, steps must be taken at once to preserve it. They did so, and during the past summer the influx of visitors has been greater than ever.

These two stages in the growth of a summer resort—its sanitary degradation and subsequent rehabitation—can now be witnessed in every phase of their development, along the entire coast-line of the State of New Jersey. This great sea-side resort, a hundred miles in length, stretching from Sandy Hook to Cape May, is rapidly growing into an almost continuous city. It harbours each summer a vast multitude from our two metropolitan cities, and from the Middle and Middle-Western States. Even as a Sanitarium in winter, the physicians of Philadelphia, during the past lustrum, have recognised the great advantages that were pointed out by Dr. John Torrey nearly a half century ago, and are sending their patients in need of change of air to Atlantic City and neighbouring points. The arid expanses of its sandy shores have become in this way one of the principal sources of income to the State. Would it not, then, be a highly remunerative policy for the State to maintain their attractiveness? As a fact, nothing of a preventative nature was done. The shallow pits, which provided surface water for drinking and other purposes when the population was sparse, were multiplied when the visitors came by thousands. Malarial and typhoid fevers were rife in spots which the sea-breezes visited every day. Only with the consequent suspicion and public alarm which threatened to empty them

of their patronage did these places tardily move in the matter of adequate sanitation ; and now the universal introduction of cemented cisterns, and the diurnal removal of garbage under the stringent regulations of local Boards of Health, attest the purpose of the great sea-side Sanitarium to retain its highly profitable reputation.

VII. *Sanitary Advantages of doing away with Illuminating Gas as a means of Illumination.*

Any process of illumination which returns to the confined atmosphere we breathe the products of combustion is theoretically open to objection. All methods of illumination up to the present time have depended on some process of rapid combustion, oxygen being withdrawn from the air, an equal bulk of carbonic acid returned to it, and oftentimes a large amount of heat as compared with the amount of light liberated in the process. If, now, we can illuminate without the subtraction of vital and the addition of mephitic air, and if we can produce an intense light without a corresponding heating of the surrounding atmosphere, we have made two steps of great hygienic value so far as the illumination and ventilation of rooms is concerned.

There is much reason for supposing that this will be soon accomplished in the wholesale introduction of the electric light. By very many roads a crowd of inventors is pushing forward to this end. The rapid destruction of the terminals, with the corresponding need of frequent adjustment, is being obviated by a variety of devices. In some the length of the carbons is made invariable, by a supply of carbon as soon as it wastes away, through deposition of carbon from a hydrocarbon atmosphere in which the electric arc is taken. In another, wasting is prevented by an entire exclusion of oxygen, and the terminals are surrounded by an atmosphere of pure nitrogen. Another experimenter separates his carbons by an intervening material over which the arc is formed, and all parts of this electric candle are burnt away at the same rate. In one of these ways, or in some other, the problem of lighting by electricity more perfectly and as cheaply as by illuminating gas will be solved and we shall have the attendant train of hygienic benefits. In the matter of street illumination the contamination of the atmosphere by gaseous products of combustion is, of course, not a matter of great moment, but in the illumination of those places of public assembly, the church, the theatre, the lecture-room, the improvement will be of much importance.

VIII. *Sanitary Surveys.*

The intimate relation between health and the configuration of the soil has been recognised from time immemorial. In truth there is reason to suppose that more practical weight was given to it in ancient than in modern states of society, for while, in the former, security from enemies and the possible exigencies of a protracted siege made it imperative to select high places capable of good drainage for city sites, the demands of commerce are now best met by towns at the lowest levels, frequently at the estuaries of rivers and marshes formed at the confluence of great streams. While the demands of commerce are inevitable, the care upon sanitary science to avert as far as possible attendant evils is not the less urgent. For this reason the rapidly increasing bulk of statistical information upon this subject is a matter of great gratulation. The geological surveys prosecuted by the State Governments, and latterly extended to the shores of the Pacific by the munificence of the National authorities, have supplied an admirable foundation. The hydrography of the sea-coast and the sea-board estuaries has been executed on a basis so broad and solid that the topography and hypsometry of the whole country can be built upon it. In addition to these we have a number of studies of the relation of topographic and geologic features to one or all the various types of disease.

Even before the inquiry instituted by the medical staff of the Privy Council of Great Britain, the extended research by Dr. Bowditch* demonstrated the intimacy of the relation between wet and retentive soils and the prevalence of consumption, these conditions of surface structure being chargeable with a thousand deaths from consumption in Massachusetts alone. Subsequently the fluviate and pond basins of Massachusetts were surveyed and mapped out by Rickwood. Staten Island having long lain under the ban of insalubrity, a number of gentlemen interested in its occupancy and improvement instituted a sanitary survey of the island, Dr. Elisha Harris and Mr. Frederick Low Olmstead examining into the more general questions involved, and Profs. Newbery and Trowbridge the structural conditions. The influence of the surface-soil and of the underlying rock, its porosity, its bedding and its joints, upon the local climate, the drainage, and the attendant salubrity,

* "Consumption in New England, or Locality one of its Chief Causes."

have served as models for the conduct of similar investigations in various other portions of the United States. Prof. Cook has been engaged for several successive summers in running a series of levels over much of the densely-populated water-sheds of the Hackensack and Passaic, with similar objects; and recently a most minute inquiry has been made into the soil, contour, and drainage of Hudson County, by Mr. L. B. Ward, C.E. He has determined how much of its area—which includes the communities of Jersey City and Hoboken, and the smaller towns of Harrison, Kearney, North Bergen, Union, Weehawken, and Bayonne—is upland, how much is marsh, what portion is rocky, what occupied by soil of various kinds, and, where possible, the nature of the sub-strata, with the population on each tract, and their condition in regard to sewerage and drainage. The ability of each variety of underlying rock, serpentine, sandstone, and trap, to carry off surface water, is considered, with the corresponding influence upon the surface temperature, dryness, and salubrity. When we consider the abrupt changes of habitat over this crowded area, it will be seen that it offers a field peculiarly favourable for the study of the effect of surface condition upon the rates and causes of mortality. Most fortunately the vital statistics of this district have been tabulated with exceptional fulness and accuracy, under the superintendence of the President of the Health Board, Dr. L. H. Elder. These statistics have been investigated by Mr. E. H. Harrison, C.E., of Jersey City, with extreme care. He has plotted upon a working map every case of fatal illness arising from insalubrious environments, each disease being indicated by a distinctive character. Other maps are in course of preparation, showing the relations of surface, contour and drainage of soil, of rock, of sewerage, and relative density of population over the same area.

IX. *Upon the Composition of the Atmosphere.*

In conclusion, I hope I may be permitted to say a few words with regard to one topic of sanitary science which for a long time has more particularly interested me, and which at present is the subject of especial study—the composition and purity of the atmosphere. As communities grow more dense, and factories multiply, the sources of aërial impurity augment in a rapidly growing ratio. In England the Government has been forced to appoint an inspector, the celebrated Dr. Angus Smith, who has made

thousands of examinations of the air in all parts of the country, and directs Government interference where persons and property are too much imperilled by atmospheric contamination. In Glasgow a city analyst has been recently appointed with this special duty. New York is already showing the effect of the sulphurous and nitrous vapours sent out from its myriad chimneys. Recently the U. S. Signal Officer, in his room at the Equitable Insurance Building, took out from a carefully wrapped package an instrument which he desired to show me, and it was hopelessly corroded by the acid vapours sent out from the tall chimney of the United States Assay Office near by. In Philadelphia there is scarcely a house-front which is not disfigured by some stain of magnesia and lime-salts, a result in part due to the acid vapours in the atmosphere. And when rains sweep down and carry with them in solution such agents, they are more powerful to corrode metal, and even stone surfaces, than would at first appear credible. When one of the normal constituents of the atmosphere is wanting—the ozone—it has largely lost its sweetening and disinfecting powers, and there is much reason for believing that the prevalence or severity of certain diseases is intimately connected with the varying amounts of ozone in the atmosphere. Unfortunately there is much difficulty in estimating the percentage of this constituent of the air, and in guarding against the disturbing influences upon our determinations of other bodies possibly present. To overcome these difficulties experiments have been on foot in the laboratory of the Stevens Institute of Technology for many months. Numerous analyses of the atmosphere, collected in various parts of the United States, have been made and recorded. They will serve as contributions towards a beginning of a chemical climatology of this country, and might, with great profit to the physician, the agriculturist, and meteorologist, be vastly extended by Government aid in connection with the Signal Service and the Department of Agriculture. We cannot do unaided as much in researches of this character as can be done in the laboratories of the Old World by Government assistance, but we can at least labour in the hope that the time is not distant when the importance of research of this kind, even if it does not end in a profitable invention, will be generally understood and generally encouraged.

IV. THE COURSE OF NATURE.*

By Prof. SIMON NEWCOMB.

IN imposing on its retiring President the duty of delivering an Address, the constitution of this Society sets no limits to his choice of a theme. Both in these and in the corresponding addresses delivered before the sister society of Great Britain, it has not been uncommon for the speaker to choose for his subject the general progress of scientific research during the year. This course is now less common than formerly, because, owing to the immensity of the field of research, it has become impossible for any ordinary mind to follow its progress in all its branches. I have thought, therefore, that a higher interest would attach to a theme chosen from the field of modern scientific thought, and, by a process in which I have been the follower rather than the leader of my own contemplations, I have been led to present to you some thoughts on the Course of Nature as seen in the light of modern scientific and philosophic research. Though I have but a single central idea to present to you, namely, that of the simplicity and universality of the Laws of Nature, yet so great is the confusion of thought which prevails on the question, What are the Laws of Nature? that it is necessary to approach my idea from more than one standpoint, and to illustrate it in more than one way.

We all know that the history of the Caucasian race, during the last three centuries, has been marked by a kind of intellectual development so entirely without precedent that some might call it miraculous; in fact, by such a development of the understanding of the course of nature as has revolutionised human society in many of its phases. You also know that this development has been marked by frequent collisions of opinion between the investigators of the material manifestations of nature on the one side (if I may be allowed to use the expression), and philosophers and theologians on the other, respecting the true theory of the course of nature. My desire in entering this field is to act the part of the peacemaker rather than that of a combatant, not sustaining any other propositions than those which are

* An Address delivered before the American Association for the Advancement of Science, St. Louis, August 22, 1878. Communicated by the author.

actually believed in by the large majority of educated men at the present time; but the confusion of thought on this subject, to which I have just alluded, is so great that—although I may combat no opinions actually held—it may be necessary to greatly modify their application, and to criticise the forms in which they have found expression.

The key-note of my discourse is found in a proposition which is fundamental in the history of modern science, and without a clear understanding of which everything I say may be entirely misunderstood. This proposition is, that Science concerns itself only with phenomena and the relations which connect them, and does not take account of any questions which do not in some way admit of being brought to the test of observation. The only universe it knows is that made known by the telescope, the microscope, and other appliances of observation. That this is the whole universe we should all be very sorry to suppose, and none more so than he who has the honour to address you. But, should I pretend to a scientific knowledge of what lies behind this visible frame, I should be acting the part of the rash speculator rather than of the cautious thinker. Only into a single field of thought do I dare to venture. When we trace the efforts of men to penetrate the secrets of Nature, we find them clearly divisible into two classes—philosophic speculation and scientific investigation. We find the objects of thought equally divisible into two classes—phenomena and their hidden causes, those unknowable entities out of which they proceed. The great progress which the last three centuries have witnessed has been wholly in the field of phenomena, and it is to this field, and to the results of scientific investigation in it, to which I ask your attention this evening. But, it is to be expected that, in this brief characterisation of our field of thought, I have failed to convey to your minds any clear conception of its boundaries. The progress here alluded to has been rendered possible only by entirely rejecting the mode of thinking about nature which was prevalent in former ages, and into which the untrained mind is almost sure to fall at the present day. The distinction will be evident to one mind at a glance, while another may be unable to comprehend it after all the explanations which it is possible to give. As my whole discourse will be misleading unless all my hearers have a clear conception of it, I shall endeavour to present you with the materials of such a conception, rather in the form of concrete illustrations in familiar language, than in that of abstract general definitions.

As one mode of expression, we might say that modern science introduces into the higher modes of thought about nature that same kind of practical good sense which characterises the successful man of business. Scientific investigation is, in a certain sense, purely practical in both its methods and its aims. There is a mental operation, with which all are well acquainted, under the familiar term "theorising;" to this operation all scientific investigation is so much opposed that the mere theoriser and essayist can never make any real advance in the knowledge of nature. To speak with a little more precision, we may say that as science only deals with phenomena and the laws which connect them, so all the terms which it uses have exact literal meanings, and refer only to things which admit of being perceived by the senses, or, at least, of being conceived as thus perceptible. This purely literal meaning of all scientific language is in strong contrast to the metaphorical and poetical forms of expression into which we are apt to fall in discourse upon abstract subjects generally, where our ideas cannot be at once referred to sensuous impressions.

We might also say that no question is a scientific one which does not in some way admit of being tested by experience. The single object of scientific research is to predict the course of nature, or the results of those artificial combinations of causes which we call experiments; and no question is a scientific one unless its solution will in some way advance this object. I must not, however, be understood as saying that the test of experience can always be immediately applied, because then no disputed question could be a scientific one. For example, the question whether man existed on the earth 50,000 years ago is a scientific one, because it is one respecting actual historic occurrence of scenes evident to the senses. It could at once be settled by simple inspection, could we in any way form a picture of the earth as it then looked, and it may actually be settled in the future by the presence or absence of sensible traces of the existence of man at those times. Should we, however, go farther, and enquire whether such men had souls, our enquiry would not be a scientific one, nor one in which science could in any way concern itself with profit. The soul can neither be seen nor in any way made evident to the senses of others. From the very nature of things, it could leave no material trace of itself to be unearthed by the geologist or antiquarian of a future age. So far are we from forming any conception even of our own souls, as sen-

sible existences, that no question affecting them, even now, is a scientific one; much less can science consider those of past generations.

There is thus a quite well-defined limit between questions which are scientific ones and those which are not scientific, and with which, in consequence, science has no concern whatever. You must not understand me as in any way claiming that questions of this last class are not worth thinking about. They include many which are of the most absorbing interest to the human race, and about which men will think the more as they become more thoughtful. But to mix them with scientific discussions will only introduce confusion of thought respecting sensible things, without in any manner advancing their solutions. The current desires that science shall consider man as something more than an animal are as unreasonable as if we wanted to make algebra a help to moral philosophy.

This limitation of all scientific research to a single specific field is something so little understood that I may have occasion to call it to mind in other connections. But there is another equally essential maxim of science which I must explain in order that you may understand the spirit which animates scientific investigation. It is that the man of science, as such, has no preconceived theories to support, but simply goes to nature to find out and interpret what she has to say according to her exact meaning. What he may desire to be true has no bearing at all on the question what really is true. Here arises the inability of men of science to view theological questions in a light which shall be satisfactory to the theologians, and the corresponding inability of the latter to appreciate the spirit in which men of science discuss the problems of life and being. We hear much at the present time of a supposed conflict between science and religion; but it is rather a conflict between two sets of men who view nature from opposite and irreconcilable standpoints. It is essential to the understanding of our theme that we should see in what this difference of view consists; I shall, therefore, endeavour briefly to explain it.

The theologian looks upon the doctrines he has been taught as something the truth of which is essential to the welfare of humanity, and, we might almost say, to the supremacy of the Creator. He thus invests them with an attribute of moral excellence, implied rather than expressed in the term orthodoxy, and looks upon those who attack them not simply as men who are mistaken, but as men who are seeking to do a great injury to the human race. Hence the

idea of weighing the arguments on both sides with entire indifference to the result is one which he cannot be expected to receive with favour, or even to readily comprehend as received by others. His idea of truth is symbolised in the pure marble statue which must be protected from contact with profane hands, and whose value arises from its beauty of form and the excellence of the ideas which it embodies. He therefore looks upon those who attack it with feelings not unlike those of the keeper of the statue upon a chemist who refuses to see anything in the statue except a lump of carbonate of calcium of peculiar form, and who wants to handle it, weigh it, determine its specific gravity and its cohesive power, and test its substance with acids. The corresponding idea of the scientific investigator is symbolised by the iron-clad turret, which cannot be accepted until it has proved its invulnerability. Instead, therefore, of being protected from violence as if it were a product of the fine arts, violence is invited. Its weak points are sought out by eyes intent on discovering them, and are exposed to the fire of every logical weapon which can be brought to bear upon them. A scientific theory may thus be completely demolished; it may prove so far from perfect that its author is glad to withdraw it for repairs or reconstruction; or it may be hammered into an entirely new shape. But however completely it may stand the fire, it maintains its position as a scientific theory only by being always in the field ready to challenge every new comer, and to meet the fire of every fact which seems to militate against it. A countless host of theories have thus been demolished and forgotten with the advance of knowledge, but those which remain, having stood the fire of generations, can show us a guarantee of their truthfulness which would not be possible under any other plan of dealing with them.

As a consequence of this way of viewing theories, the scientific man recognises no such attribute as orthodoxy in his doctrines. There is nothing at all which he says you must believe to be true as a condition of scientific recognition. There may, indeed, be many propositions to doubt which would indicate extraordinary incredulity, or downright folly, or even insanity, and he might, therefore, regard a sceptic as possessing a pitiful feebleness of intellect, and in consequence, refuse to listen to him; but he would refuse, not because the man disbelieved something which was undoubtedly true, but because he was not worth listening to. Perhaps the point which I am striving to make clear may be most readily grasped by the reflection that

science offers its highest rewards to him who will overthrow and supplant its best-established and most widely received theories. Thus the names of the men who disproved the theory of epicycles in astronomy, and the doctrine of phlogiston in chemistry, occupy the most honourable positions in the history of science. Of course, no such thing as authority in science has anything more than a provisional recognition. If a man of good repute says that he has investigated a certain subject and reached a certain result, the latter may be accepted on his authority, in the absence of other evidence. But this gives no reason at all why any one else should not reach a different result, and it would be no argument at all to cite the mere authority of the first against the second. In case of a discrepancy of this kind, the whole question would have to be re-investigated. The dictum, "It is written," has no terror whatever for the investigator of nature; he can recognise no authority for any feature in the course of nature, except nature herself as he sees her.

These principles are of so much importance in the philosophy of science that I may be pardoned for viewing them in yet another light. In reading those discussions with scientific men on certain theories recently advanced by the more advanced students of philosophic biology into which the representatives of theology sometimes enter, I have often noticed that if the representative of science propounds, discovers, or brings forward any fact or principle which seems to tell against his side of the question, the other calls it an "admission," or "concession," just as if his opponent had first selected his side for the love of it, and was then unwilling to concede or admit anything which might militate against it. Now, to go into the philosophy of the subject a little deeper than heretofore, allow me to say that the man of science professes no ability to recognise truth on sight, as he would recognise a house or an animal. The question whether any given proposition is or is not true, is necessarily to be decided by the human judgment, co-ordinating all the facts which bear upon it. There is no such thing as a revelation of scientific truths, and even if one should claim that there was, the admission or rejection of the claim would be an act of the judgment, which thus becomes the ultimate arbiter in any case. Hence a proposition is to be proved probable or true, not by anything in itself, but by a more or less long and painful examination of the evidence for and against it. Everything that can be found to militate in favour of it is put into one scale, and

everything that can be found to militate against it is put into the other. If the investigator is imbued with the true spirit of science, his search is equally vigorous for arguments to go into the two scales. When he says that the proposition is worthy of being received as true, he means, not that it bears any recognised seal of truth, but that the evidence in favour of it entirely preponderates over all that can be brought to bear against it.

You will not understand me as maintaining that every individual man of science constantly maintains this spirit of impartiality any more than every Christian constantly lives up to the highest standard of his profession. Hot conflicts have sometimes raged, and there is no reason to suppose that they have entirely ceased, even now, in which each combatant could only see one scale. But the spirit I have described is that in which science exhorts her votaries to approach every question, and in which they will constantly endeavour to approach it if they are worthy of their profession.

Let us now approach our main theme, the course of visible nature. Let me again remind you that of the two universes, the seen and the unseen, I am only going to speak of the former. We find ourselves placed in this world in the midst of a vast theatre of activity. We see an atmosphere agitated by storms; great masses of water rising in the air to form clouds, and, after falling to the earth, flowing as mighty rivers to the ocean; countless forms of vegetation rising from the earth and then returning to it; a sun supporting all life on our planet with its heat; an infinitude of chemical changes going on around us; countless stars moving through space with velocities which transcend all our conceptions. To all appearance these operations have been going on for millions of ages past, and may continue for millions of ages to come. As the thinking man contemplates them he is led irresistibly to the conclusion that they do not go on at random, but that they are joined by connecting links, or are in some way the product of knowable causes. From his earliest infancy he sees connections between them which enable him to foresee results. He finds that fire burns, that the sun warms, that food satisfies his hunger, and that heavy bodies fall with a certainty which shows the forces at play to be invariable in their action. To penetrate the mystery in which these forces are enshrouded, he has exerted the efforts of his intellect from its first dawn until the present time. What general conclusions has he reached?

From the earliest times at which man began to think, two modes of explaining the operations of nature have presented themselves to his attention. Those modes are sometimes designated as the teleological and the mechanical.

The teleological explanation of nature presupposes that her operations are akin to human actions insomuch as they are under the control of, and directed by, one or more intelligent beings having certain ends in view; that the events are so directed as to compass these ends; and, finally, that the relation of the events to the ends admits of being discovered by observation and study. This last condition is a very important one, because without it the teleological explanation of the cause of nature would not be a scientific one. The doctrine that the Author of Nature has certain ends in view, and directs the whole course of events so as to bring them about, will not enable us to explain and predict the events unless we know what those ends are. But, as I have already said, the test of scientific advance is the power of foresight—of foreseeing what result any combination of circumstances will lead to. If we always had to wait for the result, and could then only say, I know this is the result which was intended, because it has happened, no actual foresight would be possible; and however excellent the doctrine might be as a theological one, it would not admit of being tested by observation and experiment, and the question of its truth would, therefore, not admit of being settled by scientific investigation.

You may recall the remark of a satirical philosopher when he saw the gifts which those who escaped the dangers of a certain treacherous and stormy sea offered up to the goddess who had this sea at her command: "I see no offerings from those who were lost," said he. It was not till the voyager had got safely to shore that he found himself under the protection of the goddess.

It must be well understood that the teleological theory of nature, or, as it is now familiarly called, the explanation of natural phenomena by design, has two distinct forms, the scientific and the theological. These forms are not antagonistic ones; the one held by scientific men and the other by theologians; for, as you may well know, the scientific form is the one in which scientific men almost universally reject the teleological theory, while they have nothing to say against the other forms. The forms refer only to the fields to which the theory may belong, the scientific and the theological. The distinction turns on whether we suppose the ends which the Creator has in view to be dis-

coverable by scientific investigation or to be inscrutable. Only in the former case have we, as scientific investigators, anything to do with the question. The theory, as we have to consider it, is in brief this: that the course of events in inanimate nature is from time to time modified by invisible intelligences just as it is modified by man when he changes the course of a river or plants a forest.

The other explanation of nature is the mechanical one. It assumes that her processes go on in accordance with certain laws which admit of being fully comprehended by the human mind so far as their effects are concerned. Each state of things is the effect of the state which immediately precedes it, and the cause of that which immediately follows it. The course of nature is thus considered as an endless chain, of which the work of science consists in making out the forms of the links, and the modes in which they are connected. In this work we have to be concerned with two things: the general laws of nature, as they are familiarly called, and the facts or circumstances which determine the operation of these laws. This distinction is most clearly seen in human laws. Thou shalt not steal, is a law; that John has stolen, is a fact. The combined result of the law and the fact is that John is locked up in jail. So that all bodies near the earth gravitate towards it with a force directly as their mass, and inversely as the square of their existence from its centre, is a universal law of nature. The Niagara River and the precipice are facts; and the cataract is the result.

But the general explanation of the course of nature, on the mechanical theory, is not of this simple kind, because the laws of nature do not act singly, but in combination; so that the result of each is modified by the action of all the others which come into play. The law of gravitation is not that all bodies must fall, but only that they tend to fall, and, therefore, will fall unless held up by some sufficient opposing force. So long as I support this weight in my hand it does not fall, because the force of gravitation and the resistance of my hand neutralise each other. But the instant I let go the weight drops, according to a certain law known as that of uniformly accelerated velocity.

The doctrine I am endeavouring to elucidate is this: knowing a few simple laws of nature, of which gravitation is one; knowing also the arrangement of material things within the field of investigation; that is, knowing the facts, we can predict with unerring certainty what the result will be: or if we cannot predict it, it is not because of any

quality of the thing itself, but only because of the insufficiency of our powers. Moreover, these results will be, as it were, another layer of facts from which it is possible to predict new results to follow them, and so on without limit, unless some force from without intervene to change the course. If we include the whole of nature in our field, no outside facts can come in, and her course, therefore, admits of being predicted with entire certainty from beginning to end.

Now the point which I wish to bring to your attention is the revolution which modern science has brought to pass, in the opinions of mankind, respecting the relations of the two classes of causes, or supposed causes, which I have described. That all events could be explained on teleological principles, it is not likely that any one ever supposed. That the falling of heavy bodies, the running of rivers, the changes of seasons, and the revolutions of the heavens were all in accordance with mechanical laws, at least so far as the phenomena are concerned, no one ever knowingly denied. But it was thought that the action of these causes was from time to time modified by the introduction of causes of the teleological class, just as a rock might be kept from falling by the force of cohesion. The general rule has been that the more ignorant the age, the more minute and immediate was supposed to be the action of those beings who were modifying the course of nature in order to compass their ends.

As illustrating this, I might commence with the age of image worship, when the fate of the individual is supposed to be at the mercy of certain spiritual entities, symbolised by forms of wood, stone, or wax. But, leaving out of consideration ideas so different from those which prevail among us, let us come nearer home. It is not many generations since men who knew that the regular course of nature went on in accordance with mechanical laws believed, nevertheless, that occurrences of a terrific or extraordinary character were specially brought about to compass some end of Providence. Not only so, but, what is most essential to our theme, this end was supposed to be a scrutable one. The motions of stars and planets had gone on from age to age, until no new aspect of them inspired alarm. But a comet was looked upon as a messenger specially sent to give warning of a coming calamity. The scrutable end was, in this case, the warning of mankind. Ordinary cases of sickness and accident, whatever their result, always have been looked upon as a part of the regular course of events. But it

is not many centuries since the pestilence was believed to be specially sent by Heaven to punish mankind for their wickedness. Punishment and terror were here the ends which Providence was supposed to have in view. The regular daily breezes and showers were supposed to be the result of natural laws. But these laws were not supposed to be entirely adequate to the production of the tornado, which was again a special messenger, and they were suspended or their action was modified in times of extreme drought threatening mankind with famine.

These special messengers of Heaven have, one by one, yoked themselves to the car of natural law, so that I think I can hardly be wrong in saying that the supremacy of mechanical law and its adequacy to account for the whole course of nature, as we see it going on before us, is now the almost universal opinion of educated men. This revolution in human thought is, perhaps, clearly brought out in the different view we now take of certain religious observances introduced by our ancestors, whose ideas would now be considered as approaching the irreverent. Take, for example, the prayers for the right kind of weather, which we find in our prayer-books. When they were first composed and inserted, their object was a purely practical one. As the farmers now sometimes fire off cannon to make the black cloud break and discharge its contents upon the parched field, so the prayers were to be offered up in order that the aqueous vapour in the air might be made to condense and fall. That a much more exalted view of prayer than this is now taken by the more enlightened portion of the religious world, I think we have every reason to believe.

Although we can hardly entertain a serious doubt that the mechanical theory of natural operations, or, as it is sometimes called, the doctrine of the uniformity of nature, is generally acquiesced in by the mature thought of intelligent Christendom, yet objections are frequently made to it because it seems to run counter to some of our most cherished ideas. If it were not paradoxical to make the assertion, it might be said that we hold, or at least express, entirely inconsistent views on the subject. The fact is that we are pupils of two opposing schools, which are, in a certain degree, antagonistic, one of which we cannot, and the other of which we will not, give up. In one of these schools the chief teachers are observation and experience. All sentiment and emotion are banished from its curriculum, which admits only the hard realities of the outer world. The older we grow the more we see and hear of this school, and the

more unreservedly we accept its teachings. It tells us that the whole course of nature takes place in accordance with certain laws capable of expression in mathematical language; that these laws act with more than an iron rigour, and without any regard to consequences; that they are deaf to prayer or entreaty, and know no such thing as sympathy or remorse; that if we would succeed we must study them, and so govern ourselves that their action shall enure to our benefit.

The other school is that of sympathy, emotion, and religious faith. In it, as children, we receive our first teachings. It shows us ourselves placed, as it were, in a forest of mystery, surrounded by forms over which we have no control, and able to penetrate so little into the surrounding darkness that we cannot tell what shall happen to us on the morrow. It has in all ages peopled the thickets with invisible beings having an interest in our welfare or our injury, or with providential interferences designed to compass ends of which we in advance have no conception. Its teachings are nearest and most welcome in times of affliction and fear. Its objections to the teachings of the other school are heard far and wide through the land. Notwithstanding the number of forms which these objections take, their essence may be condensed into a very few sentences. The following will probably be accepted as a fair rendering of their substance.

You take a contracted and unphilosophical view of nature when you say that the world is governed by inexorable laws. These laws are not governors, but only the instruments of government by which the real governor executes his purposes. With them, but without subverting or violating them, he can reward or punish, bring on prosperity or call down disaster, according to the dictates of his sovereign will. The child and the peasant call the thunder the voice of God. The modern philosopher attempts to correct them by showing that it is the product of evaporation and of atmospheric electricity. But the view of the child is really the more correct of the two, because he ascends at once to the first cause, and thus sees further than the philosopher who corrects him, because the latter stops short at the immediate or secondary cause without even trying to raise his eyes to the higher source of power. I think I am not far wrong in giving this as the substance of the most cogent objections which may be anticipated in any quarter against the mechanical theory of the course of nature.

Now, if these views referred only to inscrutable first

causes of things, or to the intelligent but invisible substratum which underlies the whole cause of nature, we should have no occasion to discuss them, because they would lie outside the field I have assigned as that of our contemplation at the present time, and which I have sought to describe as the field of phenomena. The doctrines that all things go on in exact accordance with the will of the Creator; that he has certain ends which the laws of nature are designed to bring about; and that an intelligent cause lies behind the whole universe of phenomena, are of a class which science has no occasion whatever to dispute. If it were made clearly to appear that the field of the teachings in question was thus limited, and was entirely distinct from that of phenomena, with which alone science is occupied, there would be no occasion for dispute between the two schools. I have no disposition to throw a single stone across what I consider the sacred boundary line, nor to enter a field which I am by natural and acquired habits of thought unfitted to cultivate. As men of science let us by no means attempt to penetrate a region in which the eye of science can see nothing but darkness. If we thus subject ourselves to the imputation of being "of the earth, earthy," we may console ourselves that our edifice is firm and durable because it does not seek to rise into regions of serener air, nor to rear its dome above the clouds.

I can hardly be mistaken in saying that the objections to the mechanical theory of nature, which I have just tried to formulate, are not always confined to the field of inscrutable first causes. There is a part of the boundary line over which the stones are flying very thickly. While some of the combatants may profess to make no attack on the doctrine of the uniformity of natural law, I cannot but think that these professions often arise from a misapprehension of the scientific side of the question. Indeed, I must confess that I have met with a difficulty from my inability to form a clear idea of the views really entertained by the school now under consideration. I have made a somewhat careful study of some of the most elaborate works of the writers of the theological school, devoted to this very topic, and I have left them without being able to decide in my own mind whether the writers do or do not hold unreservedly to the mechanical theory of the course of nature. That nearly all intelligent men really believe in this theory, at least so far as the present time and dispensation are concerned, we have abundant reason for believing. Nor is there even among advanced theologians any lack of profession of a belief in the

uniformity and supremacy of the laws of nature. But when thinkers of the other school maintain the doctrine, and trace it to its logical consequences, undisguised by sentimental language or figure of speech, they are met with criticism which I can account for only by supposing that the theologian understands by laws of nature something different from what is understood by the man of science.

Let us try to condense the questions at issue into the smallest possible space. The scientific philosopher maintains that the natural course of events goes on in invariable accordance with certain knowable laws. He asks the theologian in the words of Pope:—

> "Think'st thou like some weak Prince the eternal cause
> Prone for his favourites to reverse his laws?
> Shall burning Ætna, if a sage requires,
> Forget to thunder and recall her fires?
> On air or sea new motions be impress'd,
> O blameless Bethel, to relieve thy breast?
> When the loose mountain trembles from on high
> Shall gravitation cease if you go by?
> Or some old temple, nodding to its fall,
> For Chartres' head reserve the hanging wall?"

To all these questions the other answers no, and thus all occasion for dispute ought to end. But it does not end, by any means; for he proceeds to criticise the views of the questioner on the ground of their narrowness, and to inform him that the Creator can (and, by implication, that he does) so arrange things that any result he may wish shall be brought about by the action of natural laws themselves. "We do not expect Ætna to recall her fires when a sage is near; or the air and ocean to acquire new motions to preserve a saint from danger." . . . "Should these individuals not be rushing recklessly against the known laws of Heaven, or should it be the will of God to preserve them, it will be found that provision has been made for their escape, and that not through the powers of nature disobeying their own laws, but through other powers in nature opportunely interposing to stop, to turn aside, or otherwise to modify their operation."

Now, always supposing that such remarks as these are intended to apply to the domain of sight, hearing, and understanding, they differ fundamentally from the scientific theory in their view of what constitutes the laws of nature. The school seems to look upon causes and effects in nature as belonging to two different classes of things. They see an immense collection of causes, to each of which the appropriate effect is tied. So long as the cause is followed by its

effect, the laws of nature are satisfied. So if the Ruler wants to reward, punish, kill, or rescue, he has only to bring into operation the appropriate cause at the proper moment; the natural effect follows, and His will is executed without any violation of the laws of nature. I am not sure that this is an exact statement of the views to which I refer; but it is the best I can gather from the study of the forms in which they have found expression. Supposing this to be the view really entertained, it is essentially different from that held by the scientific philosophy. The course of nature as it presents itself to the eye of science is not a collection of isolated causes, each with its effect attached to it, but it is rather to be symbolised by a chain in which each link is connected with the link which precedes it and with the one which follows it. At each moment of time the state of the universe is the effect of the state which immediately precedes it, and the cause of the state which immediately follows. There are no such things as distinct causes and effects, but only laws of progress which connect the successive links of the seemingly endless chain.

As an illustration of this, let us take the falling of the rock. To the mere observer there is no evident reason why it should fall at one time rather than another; he may, therefore, feel that there is room for speculation as to the cause which made it fall at the exact moment it did. But science teaches that it will fall at the very moment when the cohesive attraction which binds it to the mountain behind becomes less than the weight of the rock. We might suppose a power to so adjust the causes which effect the cohesion that the rock shall fall at some desired moment. But any such adjustment would be as complete a change of the course of nature as if the power should hold the rock up after it had begun to fall. The natural processes by which the cohesion of the rock is slowly diminished, though largely hidden from our view, are governed by laws as precise in their action as those which regulate the motion of the planets. The water which falls from the clouds slowly percolates through the ground and enters a crack in the supporting mass. It wears it away at a rate dependent on the solubility of the material and the quantity of water which falls. A constant but certain molecular action goes on without ceasing between each molecule of water and each molecule of rock. The strength of the latter is thus weakened according to some law admitting of precise mathematical statement. Thus a mind possessed of sufficient mathematical ability, knowing how much water runs over

the rock from time to time, and knowing also the laws of molecular action between the rock and the water, could determine long in advance the very moment at which the rock would fall.

Going back another step, we see that the quantity of water which runs over the rock depends on antecedent circumstances in the same way, namely, upon the quantity of the rainfall and the arrangement of the crevices in the ground. However the latter may have been produced, the cause is still another link in the endless chain which we can trace back to preceding links as far as we please. Equally is the rainfall a fixed element, determined by the course of the winds and the amount of moisture which they carry. Thus we have a network of causes too complicated for the human mind to trace in detail, but which the philosophy of science teaches us act with mathematical certainty. No tempering, modifying, or adjusting action comes in at any stage of the process, so far as we can see; if we admit such action we have to keep placing it farther back as our knowledge increases.

Now there is one feature of these causes, the admission or rejection of which constitutes the main point of difference between the two schools of thought which I have before indicated. All are agreed that the course of nature is determined by what we may call causes or laws, but all are not agreed as to the scope of action of these laws. The great and distinguishing feature which the school of science recognises, and which the other school does not recognise, is that all the laws of nature act without any scrutable regard to consequences. I qualify my statement by the word scrutable, because it is entirely outside the pale of scientific research to speculate upon possible inscrutable ends in nature. This being a subject of which the man of science, speaking as such, can affirm nothing, so he can deny nothing. Having found that no trace of regard for consequences can be seen in the mode of action of the laws which he investigates, but that the whole course of things, so far as his eye can penetrate, may be explained and predicted without supposing any such regard, the demands of science are satisfied, and he must there stop.

Let me illustrate this by going over the train of thought which has just occupied us in the opposite direction, starting from the rainfall, and tracing the succession of causes to the fall of the rock. The spot at which each drop of rain shall fall is determined by antecedent conditions entirely, by gravitation, and the winds. The drop neither

seeks nor avoids the crevices, never asks in any way what shall be its destiny after it reaches the ground. It strikes the ground wherever gravity and the winds bring it, percolates through the soil according to the law of least resistance, and dissolves the rock according to the laws of chemical affinity, without any respect to the consequences, immediate or remote. At length a moment arrives at which the cohesive force of the rock becomes less than the weight which urges it downward. This moment is fixed entirely by antecedent circumstance, such as the solubility of the rock, and the amount of water which percolates over it. At this very moment the rock begins to fall. It falls sixteen feet the first second, three times that distance the next, and so on, according to the mathematical law of falling bodies, without any respect to the lovely character of the beings it may destroy, or the disasters with which it may crush the fondest hopes of men. The region may be the wilderness; the passer-by may be a babe in its nurse's arms, an angel of charity, fulfilling her mission of good will, or a murderer aiming the deadly blow at his victim; but under no circumstances can we see that these conditions in any way affect the chain of causes which lead to the falling of the rock, or cause it to wait a moment, or swerve a hair's breadth from its inevitable course.

According to the theory of the course of nature, which I am trying to elucidate, the chain of causes which we have described, each cause acting according to antecedent conditions, but without any regard to consequences, is the type of the whole course of inanimate nature, as far in space as the telescope can penetrate, and as far back in time as the geological record can be deciphered. An essential feature of the theory is, that the laws which connect the several links of the chain, and thus determine the progress of events, do not possess that character of inscrutability which belongs to the decrees of Providence, but are capable, so far as their sensible manifestations are concerned, of being completely grasped by the human intellect, and expressed in scientific language. Without this the theory would have no practical bearing whatever; because, to say that the course of events is fixed, but by laws which we can never grasp, would give us no clue at all to learning what that course shall be, and would be equivalent to telling us that it is enshrouded in the same impenetrable mystery with first causes. A very important feature of the progress of science is found in the constant resolution of the laws of nature into more simple and elementary ones, until we reach principles

so simple that it is impossible to analyse them farther. Let us take as an instance of this the laws of the celestial motions. When Kepler discovered that the planets moved round the sun in eclipses, having the sun in one focus, he found what were, for his time, simple and elementary laws. They were entirely comprehensible, admitting of being expressed in mathematical language. They enabled him to predict the motions of the planets, and, so far as the intellect of the time could penetrate, they could not be resolved into more simple expressions.

But when Newton appeared on the scene, he showed that these and other laws could be expressed in the simple and comprehensive form of gravitation of every particle of matter toward every other particle with a force inversely as the square of the distance which separates them. All the laws of planetary motion which had before them discovered, were shown to be reducible to this one simple law, combined with certain facts respecting the directions and velocities of the planetary motions. The most essential of these facts is that the velocities of the planets in their orbits are such that under the influence of the sun's gravitation, these orbits are nearly circular.

By this grand generalisation Newton reduced the laws of the celestial motions to a form so elementary, simple, and comprehensive, that no further reduction seems possible in our state of knowledge. Attempts have been made to show that gravitation is itself the result of discoverable causes, but they appear to me entirely unphilosophical, since the causes into which they would resolve gravitation are more complex than gravitation itself. But for our present purpose it is not necessary to concern ourselves whether gravitation may arise from some more subtile principle as yet undiscovered. The point which I wish you to grasp is the entire comprehensibility of the law as it now stands. There is no mystery surrounding it. When I say that any body left unsupported will fall toward the centre of the earth until it meets with the earth itself, or some other obstacle to its farther fall, you know exactly what I mean, and what are the results of the law which I enunciate. In a certain sense we might say that the laws of nature are simply *general* facts, distinguished from special facts by their dependence upon certain antecedent conditions. Considered as such, there can never be any doubt as to their meaning or results. There is no profound philosophy involved in their action or expression any more than there is in such statements as that all unsupported bodies fall toward the centre of the

earth; that gunpowder, when touched by fire, suddenly changes to an incandescent gas; that water, at ordinary pressure, changes to steam at a temperature of 212°.

Now, scientific investigators are earnestly endeavouring, each in his own sphere, to do for the whole of nature what Newton did for the laws of planetary motion, to find and announce the elementary principles which connect all the links of the endless chain which symbolises her course. The student of chemistry cannot doubt that the innumerable properties of the various compounds which he studies arise from the play of certain attractive and repulsive forces among the elementary molecules of the matter of which these compounds are formed. Could he only learn the law according to which these forces act, chemistry might become very largely a deductive science, and the properties of compounds might be predicted in advance, as the astronomer predicts the conjunctions of the planets. The idea now entertained by those who see farthest in this direction is that all the physical properties of matter depend upon, and may be reduced to, certain attractive and repulsive forces acting among the ultimate atoms of which matter is composed.

It may also be supposed that all the operations of the vital organism, both in men and animals, depend in the same way upon molecular forces among the atoms which make up the organism. The operation of forces unknown to chemistry must, indeed, be presupposed, but there is no reason to suppose that these forces are less simple than chemical ones. Some would even go so far as to explain the facts of consciousness in this way. The philosophy of this explanation belongs, however, to another department of thought—that of scientific materialism—into which we cannot at present enter.

The most startling attempts in the direction I have indicated are those which are designed to show that those wonderful adaptations which we see in the structure of living animals, and which in former times were attributed to design, are really the result of natural laws, acting with the same disregard to consequences which we see in the falling rock. The philosophy of Darwinism, and the theory of evolution, will be at once brought to your mind as forming the modern system of explanation tending to this result. On these theories the eye was not made in order to see, nor the ear in order to hear, nor are the numberless adaptations of animated beings to the conditions which surround them in any way the product of design. Absurd as this theory appears at the first glance, and great as is the anxiety to

secure its rejection, the question of its truth is to be settled only by a careful scientific study of the facts of nature, and the laws of hereditary descent. The principle which is to aid in its settlement is universally admitted in quarters where it is fully understood. We are not to call in a supernatural cause to account for a result which could have been produced by the action of the known laws of nature. The question then is whether these laws of hereditary descent and of natural selection are adequate to account for the gradual growth of such organs as the hand, the eye, and the ear, and for all the adaptations which we see in nature. If they are it would be idle to call in any other cause, except we place it behind the laws, and if we place it behind those laws we must equally place it behind all others. Of course, such a cause lies beyond the field of sight, and does not, therefore, belong to scientific observation. Granting the theory, then, so far as the eye of science can penetrate the whole result is brought about by laws acting in seemingly blind disregard of consequences.

Let us now turn once more to the theory of scrutable design, which supposes, at least, the occasional action of causes which the human intellect can perceive to have been intended to produce certain effects, such as the salvation of the righteous, the punishment of the wicked, the warning of the indifferent, or the preservation of the race. Studying this theory from the purely scientific standpoint, in all the varying forms in which history presents it, we see its distinguishing feature to be the idea of causes acting so as to bring about certain results.

When Pallas inspired Diomed with renewed strength, and gave superhuman accuracy to his aim, it was in order that he might be able to pierce his Trojan enemies. Ordinary investigation might fail to show that his hand trembled less than usual as he raised his javelin, but the goddess took care that the last tremulous motion of his hand, as the weapon left it, should be in the direction to send it into the breast of the foe. The utterances of the oracles were determined, not by the past or the present, but by events still in the future. The blazing comet appeared, not in obedience to a chain of causes commencing with the creation, but in order that man might be warned of the coming calamity. When the prayers of the righteous averted the coming storm, the cloud moved aside in order that their fields and houses might be saved, and when they brought down the gentle rain upon the parched fields, the rain fell in order that famine might be averted.

These supposed causes differed from what enlightened minds now understand by the term Providence, in being amenable to scientific investigation, and in not being included in the regular chain of natural phenomena. The designs of Providence are inscrutable, but those of Pallas and Juno were not. Careful experimental investigation, such as might have been undertaken by a Helmholtz of that time, would have sufficed to show just how Pallas wanted the spear thrown, if the view of the Homeric age was the correct one. When the King died, or the enemy was victorious, men thought they knew exactly why the comet appeared when it did.

These views having so far vanished into thin air, I do not see how we can avoid recognising the reality of the revolution which modern science claims to have made in the views of men respecting the course of nature. And yet, as I have already shown, there are many tendencies in our being which make us unwilling to admit the revolution, and lead many to look upon the old theory as correct, provided it were only considered as tracing causes to the will of the Creator. On what is this view founded at the present time? Entirely, it seems to me, in ignoring the distinction between the scrutable and the inscrutable, between the seen and the unseen worlds. Science has, to a greater or less degree, banished final causes from the visible universe; but they act with undiminished vigour in the invisible one. Such a translation may not be a great revolution in thought, from a theological point of view, but it certainly is from a scientific standpoint, which considers only visible things.

I can readily imagine your asking if teleological causes can be really considered as absolutely banished from the whole domain of visible nature, if, considering how limited our knowledge, and how vast that part even of the visible universe which we do not know, it is not rash to assert that we know the true theory of nature, even in the field of phenomena. This question may lead us to look a little more carefully than we have hitherto done upon the exact standing of the doctrine of the uniform course of nature according to antecedent causes, and the relation of this doctrine to modern scientific investigation. And this leads me to say that it would be entirely unphilosophical to regard the revolution I have described as a scientific discovery or induction. It may be doubted whether the scientific mind is really any less disposed to believe in final causes than the ordinary mind. Nor can the theory that the course of nature is symbolised by the chain of cause and effect, as I

have described it, be considered as a product of modern investigation simply, or as belonging especially to the present age. It is a theory which has been, in a limited sphere, recognised by all men at all times. The reason why modern science has so greatly extended its scope is that modern science has acquired a vastly more extended view of nature than has before been obtained. One of the most curious and suggestive features of the teleological theory has been that the action of teleological causes has always been ascribed to operations into which human investigation could not penetrate, although their ultimate effects might be plainly seen. Whenever the subject becomes so well understood that the chain of natural causes can be clearly followed, miracles and final causes cease, so far as the scientific explanation of things is concerned. That a ball or spear thrown in one direction would bend its course into an entirely different direction no one ever supposed. Homer never imagined Pallas as changing the course of the javelin after it had left the hand of Diomed. But those states of the nervous system which result in a certain and accurate aim or in a tremulous or uncontrolled arm, lay beyond the pale of physiological knowledge in the time of Homer; so here it was that the goddess intervened. When nervous action became fully understood, the final cause receded, and took refuge in some deeper arcanum of our ignorance. Jove was never expected to make thunder and rain without clouds, nor was the falling of the rain ever ascribed to his interference, because every one believed that if the drops were once formed they would fall at once to the ground, without any action on his part. But the mixing currents of moist and cool air, and the processes of condensation which lead to the formation of rain and electricity, were not understood, so here Jupiter had a chance to work unseen by man. When the mode in which clouds were formed was once understood, the god of thunder left his seat upon Mount Olympus for a more distant abode. From the earliest historic times, the man who took a large dose of poison has died, as a matter of course; neither good nor evil spirit had anything to do with it; but if brain disease bereft him of reason, the malevolence of an evil spirit was called in to account for the result.

Now, I beg you to notice that in all these cases, the only distinction we can make between those effects which were supposed to be produced by natural causes and those which were produced by the will of some higher power, acting with a scrutable end in view, is this: in the first class

of cases we can clearly see the effect to have been produced by the action of natural causes, and in the second we cannot. This distinction, depending as it does upon the extent of our knowledge, cannot be regarded as a logical one. Yet, in so far as a belief in that class of final causes which we have been considering exists at the present day, I see no other definition of the limits within which these causes are supposed to act. Let us take an illustration from the plague now desolating our southern cities. No one would believe that under any circumstances any superior power would build a yellow fever hospital and supply it with the best medicines. If we should say that the prayers of the whole nation for the immediate erection of such buildings would have no effect whatever, we should not be accused of unbelief or irreverence in any quater, for everyone would fully agree with us. But there are great numbers of people who believe that, if the whole nation should pray for frost, frost might be sent in answer to prayer when it would not have come otherwise. And to many who do not share this belief, the denial of any possibility of an influence of this kind would seem to savour much more strongly of unbelief, irreligion, or irreverence, than the denial that Providence would build a hospital without human hands. And yet, if the scientific philosophy be correct, the providential production of frost would be as miraculous and as incredible as the providential erection of a hospital in a single night without human hands. The temperature of the air, and the amount of moisture, it shall have in any given place, a day, or month, or year from the present time, is as completely fixed by the present state of things, and by the laws of evaporation, condensation, and motion of gases, as is the position of the heavenly bodies. The first deposition of frost will be determined by forces now at play, and any deviation from the inevitable action would be a miracle of the same kind as pieces of timber hewing themselves into shape, and putting themselves together, untouched by man. Please notice that this similarity between the two states of things is entirely independent of any philosophical theory of natural causes. All we claim is that the laws which determine the motion of the air, the formation of clouds, the fall of rain, and the deposition of frost, are, with respect to their certainty of action, of the same class with those which determine the position, the movements, and the cohesion of a stick of timber. If you claim that both classes of causes are the acts of the Creator, we have nothing to say against it; all we say is that you must interpret his acts in the

same way in the two cases. You must not claim that he will produce heat or cold by a fiat of an arbitrary will, unless you also claim that He will build the hospital or leave it unbuilt according to a similar fiat. Nor is it of any avail to say that you know it to be His will that the hospital shall remain unbuilt unless man undertakes it. We can, in reply, maintain that we know it to be His will that the course of nature shall go on unchanged, no matter how it may affect human interests.

It thus appears that the dividing line between mechanical and final causes, as drawn by the human mind in all ages, has not been fixed by any absolute criterion, but only near the limits of the knowledge possessed by each generation. Science has extended the line entirely beyond ordinary mental vision, not by introducing any new theory of nature, but by extending the boundaries of exact knowledge, and with them, of the field in which, by common consent, final causes do not admit of being traced. The telescope has revealed to us a universe compared with which that known to ancients is but an atom, and geology has opened up to our view a vista of ages in which the lifetime of our generation is hardly more than a moment. And thus final causes have taken their flight from a vast region in which they before lay hid in obscurity. You may now ask, have they simply taken refuge in the more distant but vastly wider circumference which now marks the boundaries of our knowledge, or are we to suppose them entirely banished from nature? This is entirely a question of intuition, and not at all of scientific investigation. I have described the scientific theory of nature as not admitting scrutable final causes at all, but as claiming that the law of the falling rock is symbolic of all her operations. But I think this is a view towards which philosophers have always inclined. We must always expect that men will incline to this view in proportion to their familiarity with the material side of nature. At the same time it is evident to all that there must have been a beginning of things, and that nature could not have commenced herself. We have, therefore, a wide belt left between the origin of nature and the boundaries of our knowledge in which we may suppose the inscrutable cause to have acted. Here we reach questions of philosophy which lie outside of our field, and which, therefore, we cannot now stop to consider.

The exact bearing of the subject will be better understood by condensing what has already been said so as to present the whole in a brief space.

1. When men study the operations of the world around them, they find that certain of those operations are determined by knowable antecedent conditions, and go on with that blind disregard of consequences which they call law. The criterion for distinguishing these operations is that their results admit of being foreseen. They also find certain other operations which they are unable thus to trace to the operation of law.

2. Men attribute this latter class to invisible anthropomorphic intelligences, having the power to bring about changes in nature, and having certain objects, worthy or ignoble, in view, which they thus endeavour to compass. Men also believe themselves able to discern these objects, and thus to explain the operations which bring them about. The objects are worthy or ignoble according to the character of the intelligences, which again depend upon the state of society. In ancient times they were often the gratification of the silliest pride or the lowest lusts.

3. As knowledge advances, one after another of these operations are found to be really determined by law, the only difficulty being that the law was before unknown or not comprehended, or that the circumstances which determined its action were too obscure or too complex to be fully comprehended.

4. Final causes having thus, one by one, disappeared from every thicket which has been fully explored, the question arises whether they now have, or ever had, any existence at all. On the one hand it may be claimed that it is unphilosophical to believe in them when they have been sought in vain in every corner into which light can penetrate; on the other hand, we have the difficulty of accounting for these very laws by which we find the course of nature to be determined. Take, as a single example, the law of hereditary descent. How did such a law—or rather how did such a process, for it is a process—first commence? If this is not as legitimate a subject for enquiry as the question how came the hand and the eye into existence, it is only because it seems more difficult to investigate. If, as the most advanced scientific philosophy teaches, creation is itself but a growth, how did that growth originate? We here reach the limits of the scientific field, on ground where they are less well-defined than in some other directions, but I shall take the liberty of concluding my remarks with a single suggestion respecting a matter which lies outside of them. When the doctrine of the universality of natural law is carried so far as to include the genesis of living beings, and the adaptations

to external circumstances which we see in their structure, it is often pronounced to be atheistic. Whether this judgment is or is not correct I cannot say; but it is very easy to propound the test question by which its correctness is to be determined. Is the general doctrine of causes acting in apparently blind obedience to invariable law in itself atheistic? If it is, then the whole progress of our knowledge of nature has been in this direction, for it has consisted in reducing the operations of nature to such blind obedience. Of course, when I say blind you understand that I mean blind so far as a scrutable regard to consequence is concerned—blind like justice, in fact. If the doctrine is not atheistic, then there is nothing atheistic in any phase of the theory of evolution, for this consists solely in accounting for certain processes by natural laws. I do not pretend to answer the question here involved, because it belongs entirely to the domain of theology. All we can ask is that each individual shall hold consistent views on the subject, and not maintain the affirmative of the question on one topic and the negative on another. My object in presenting the views I have has been not so much to propound a new theory as to promote consistency, precision, and independence of thought among those who discuss the subject.

V. PERUVIAN ANTIQUITIES.*

By E. R. Heath, M.D., Wyandotte, Kas.

ON the Peruvian coast in ancient times, as now, nearly every structure was made of adobes or sun-dried brick, while in the mountains stone was used instead. The adobe ruins present nothing of beauty, architecturally. The subject for wonder is their immensity and number. Go where you will relics of the past meet your eye, either in ruined walls, water-courses, terraces, or extensive lands covered with the *débris* of pottery.

Take, for example, the Jequetepeque valley. In 7° 24′ S. latitude you will find on recent maps the port of Pacasmayo. Four miles north, separated from it by a barren waste, the

* Kansas City Review of Science and Industry, November, 1878.

river Jequetepeque empties into the sea. The bottom lands of the river are from 2 to 3 miles in width, with a southern sloping bank, and the northern a perpendicular one nearly 80 feet high. Beside the southern shore, as it empties into the sea, is an elevated plat one-fourth of a mile square and 40 feet high, all of adobes. A wall 50 feet wide connects it with another, a few hundred yards east and south,—that is, 150 feet high, 200 feet across the top, and 500 at the base, nearly square. This latter was built in sections of rooms 10 feet square at the base, 6 feet at the top, and about 8 feet high. These rooms were afterward filled with adobes, then plastered on the outside with mud, and washed in colours. All of this same class of mounds—temples, to worship the sun, or fortresses, as they may be—have on the north side an incline for an entrance or means of access. Treasure-seekers have cut into this one about half-way, and it is said 150,000 dollars worth of gold and silver ornaments were found. In the sand, banked up behind the wall and mound, many were buried, as the thousands of skulls and bones now exposed prove; thrown out by the hunter of huacos, as the pottery is called, huaca being the name given to these cemeteries. Each body has buried with it a vessel or water-craft, and a pot with grains of corn or wheat, and it is supposed the drinking-vessel was filled with "chicha," a fermented drink made from corn or pea-nuts. Beside these were many ornaments of gold, silver, copper, coral and shell beads, and cloths. On the north side of the river, on the top of the bluff, are the extensive ruins of a walled city, 2 miles wide by 6 long. Within the enclosure are the relics of two large reservoirs for fresh water. The clay from which these adobes were made was found at least 6 miles distant.

Follow the river to the mountains. All along you pass ruin after ruin and huaca after huaca. At Tolon, a town at the base of the mountains, the valley is crossed by walls of boulders and cobble stones, 10, 8, and 6 feet high, 1 foot to 18 inches wide at the top, and 2 to 3 feet at the base, enclosing ruins of a city one-fourth of a mile wide and more than a mile long. The upper wall has projecting parts at the entrances, with port-holes, evidently serving as sentry-boxes. At this point the Pacasmayo Railroad enters the Jequetepeque valley. For 8 miles back it crosses a barren sand plain of more than 15 miles in length, covered with ruined walls, water-courses, dead algaroba and espino trees, with fragments of pottery and sea-shells, even to 9 feet in depth mixed with the sand. The base of the mountains

have, in good state of preservation, many thousand feet of an old water-course, while their sides to the perpendicular parts are lined with terraces. This water-course took its head from a ravine now dry, and, even beyond the memory of the oldest inhabitants, except in one or two cases, never carried water. It can be traced as far as Ascope, 45 miles south. Five miles from Tolon, up the river, there is an isolated boulder of granite, 4 and 6 feet in its diameters, covered with hieroglyphics. Fourteen miles further, a point of mountain at the junction of two ravines is covered to a height of more than 50 feet with the same class of hieroglyphics—birds, fishes, snakes, cats, monkeys, men, sun, moon, and many odd and now unintelligible forms. The rock on which these are cut is a silicated sandstone, and many of the lines are an eighth of an inch deep. In one large stone there are three holes, 20 to 30 inches deep, 6 inches in diameter at the orifice and 2 at the apex, and, although polished as porcelain, these markings extend even to the bottom. The locality is of no importance; the stones as Nature placed them; why, then, was so much labour and time expended upon them?

At Anchi, on the Rimac river, upon the face of a perpendicular wall 200 feet above the river-bed, there are two hieroglyphics, representing an imperfect B and a perfect D. In a crevice below them, near the river, were found buried 25,000 dollars worth of gold and silver. When the Incas learned of the murder of their chief, what did they do with the gold they were bringing for his ransom? Rumour says they buried it, and many places are pointed out and thousands of dollars spent in useless search for the lost treasure. May not these markings at Yonan tell something, since they are on the road and near to the Incal city? Eleven miles beyond Yonan, on a ridge of mountain 700 feet above the river, are the walls of a city of 2000 inhabitants. A perilous ascent on hands and knees is now the only way to reach it; however, on the opposite side of the river are similar ruins, but easy of access. A remnant of a stone wall, 10 and 12 feet high, built of small flat stones and without mortar, probably at one time served as river protection and against the tribe on the other side, there being a tradition that two powerful chiefs occupied these cities and were ever at war. The dead were buried in sepulchres, using large boulders as the top, while stone walls divided the space beneath into compartments. Six and twelve miles further are extensive walls and terraces. Three miles north of the latter place are the rich silver mines of Chilete, for-

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merly worked by the Indians, who left excavations 200 and 300 feet deep, and must have taken out quantities of silver. A company with a paid-up capital of half a million is now working them.

Leaving the valley at 78 miles from the coast, you zigzag up the mountain side 7000 feet, then descend 2000, to arrive at Cajamarca, or Coxamalca of Pizarro's time. Here and there all the way you find relics of the past. In a yard off one of the main streets, and near the centre of the city, is still standing the house made famous as the prison of Atahualpa, and which he promised to fill with gold as high as he could reach, in exchange for his liberty. Like all their stonework, the walls are slightly inclined inward, uncemented, built of irregular stones, each exactly faced to fit the next. The floor and porch are cut out of the solid stone, 2 and 3 feet deep, as the still intact remnants of stone pillars of the same rock show. The hill from which the stone for the walls was taken is near by. On its top a large stone in the shape of a chair bears the name of "Inca's chair," and the Indians say it was the king's custom to sit here every morning and salute the sun as it rose above the horizon. The two large places excavated out of the rock on the hill-side, and now used as reservoirs for the city, were of ancient make. Three miles distant, and across the valley, are the hot springs, where the Inca was encamped when Pizarro took possession of Cajamarca. Part of the wall is of unknown make (that encloses the baths). Cemented, the cement is harder than the stone itself. At Chocofan, 9 miles from Pacasmayo, on the line of the railroad, a barren, rocky mountain, 1200 feet high, is encircled 400 feet from its top by a stone wall 8 or 10 feet high. From its northern side, running nearly north-west, is about 5 miles of the coast road of the Incas. Perfectly straight, it is 20 feet wide, and walled on both sides by round stones piled to a height of 3 and 4 feet, 3 feet wide at the base and 2 at the top, uncemented. At Chepen, a station near the terminus of the branch of the Pacasmayo Railroad, is a mountain with a wall in many places 20 feet high, the summit being almost entirely artificial. In the sand at its base is one of Peru's most extensive "huacas," and from which some of the finest pottery and ornaments have been taken.

Fifty miles south of Pacasmayo, between the seaport of Huanchaco and Truxillo, 9 miles distant, are the ruins of "Chan Chan," the capital city of the Chimoa kingdom, which extended, when conquered by the Incas, from Supe

to Tumbez, or over nearly the northern half of the coast of modern Peru. The road from the port to the city crosses these ruins, entering by a causeway about 4 feet from the ground, and leading from one great mass of ruins to another; beneath this is a tunnel. Be they forts, castles, palaces, or burial mounds called "huacas," all bear the name "huaca." Hours of wandering on horseback among these ruins give only a confused idea of them, nor can old explorers there point out what were palaces and what were not.

To the right is the "Huaca of Toledo," to the left "Bishop's Huaca." The large square enclosures, shut in by wedge-shaped walls of adobe, 20 to 25 feet high, have nothing of an entrance into them that would be defined as a palace-gate. A half-a-dozen of these, at least, are among the ruins. Within some of them are large square mounds or burying chambers, many of which have been opened and rifled of their contents. These are plastered at the ceilings. Beside the so-called "huacas" already mentioned, there is another on the left side of the road called by the Spaniards "the Mass." On many of the walls is some excellent stucco-work. Excellent as regards the material of which it is made, more than with reference to its style of art. There is not a single grain of disintegration in the parts that surround the walls of the chamber, although it is half an inch high above the ordinary plaster in which it is done, nor the slightest impairment in its integrity during the many centuries it has stood exposed to the elements. The highest enclosures—those of adobe brick, up to 30 feet, with a base of 15 feet, on the right hand of the city as you advance toward Truxillo, between that town and the "Toledo huaca"—must have cost an immense amount of labour, and needed a large number of hands for their erection. Inside some of them, besides the square mounds, are narrow passages, not more than a yard in width. In others are squares, wherein are visible, though now filled with clay, the outlines of water-tracks. On this side are the principal burial mounds, some having stairs of adobe.

In the city of Truxillo there exists, in the records of the municipality, a copy of the accounts that are found in the book of Fifths of the Treasury, in the years 1577 and 1578, referring to the "Huaca of Toledo." The following is a condensed inventory:—

First.—In Truxillo, Peru, on the 22nd of July, 1577, Don Garcia Gutierrez de Toledo presented himself at the royal treasury, to give into the royal chest a fifth. He brought a bar of gold 19 carats ley and weighing 2400 Spanish dollars,

of which the fifth, being 708 dollars, together with 1½ per cent to the chief assayer, were deposited in the royal box.

Second.—On the 12th of December he presented himself with five bars of gold, 15 and 19 carats ley, weighing 8918 dollars.

Third.—On the 7th of January, 1578, he came with his fifth of large bars and plates of gold, one hundred and fifteen in number, 15 to 20 carats ley, weighing 153,280 dollars.

Fourth.—On the 8th of March he brought sixteen bars of gold, 14 to 21 carats ley, weighing 21,118 dollars.

Fifth.—On the 5th of April he brought different ornaments of gold, being little bells of gold and patterns of corn-heads and other things, of 14 carats ley, weighing 6272 dollars.

Sixth.—On the 20th of April he brought three small bars of gold, 20 carats ley, weighing 4170 dollars.

Seventh.—On the 12th of July he came with forty-seven bars, 14 to 21 carats ley, weighing 77,312 dollars.

Eighth.—On the same day he came back with another portion of gold and ornaments of corn-heads and pieces of effigies of animals, weighing 4704 dollars.

The sum of these eight bringings amounted to 278,174 gold dollars or Spanish ounces. Multiplied by sixteen gives 4,450,784 silver dollars. Deducting the royal fifth—985,953·75 dollars—left 3,464,830·25 dollars as Toledo's portion.

Even after this great haul, effigies of different animals of gold were found from time to time. Mantles also, adorned with square pieces of gold, as well as robes made with feathers of divers colours, were dug up. There is a tradition that in the huaca of Toledo there were two treasures, known as the great and little fish. The smaller only has been found.

Between Huacho and Supe, the latter being 120 miles north of Callao, near a point called Atahuanqui, there are two enormous mounds, resembling the Campana and San Miguel, of the Huatica Valley, soon to be described. About 5 miles from Patavilca (south, and near Supe) is a place called "Paramonga," or the fortress. The ruins of a fortress of great extent are here visible; the walls are of tempered clay, about 6 ft. thick. The principal building stood on an eminence, but the walls were continued to the foot of it, like regular circumvallations; the ascent winding round the hill like a labyrinth, having many angles, which probably served as outworks to defend the place. In this neighbourhood much treasure has been excavated, all of which must have been concealed by the prehistoric Indian, as we have

no evidence of the Incas ever having occupied this part of Peru after they had subdued it.

From Lima, north, along the coast, the Ancon and Chancay Railroad is built. Ancon, 18 miles from Lima, is a favourite summer sea-side resort. Just before reaching Ancon, the railroad runs through an immense burying-ground, or "huaca." Make a circuit of 6 to 8 miles, and on every side you see skulls, legs, arms, and the whole skeleton of the human body lying about in the sand. Legs attached to pelvis, and bent up, still with mummified skin on them; arms in the same state; relics of plaited straw, forming coffin swathes; pieces of net, of cloth, and many otner such accompaniments of funeral accessories. Some water crafts of very superior quality have been obtained from these graves. Of these there are three different forms, in places separated a short distance from each other, but each style having its defined outline of *locale*. As to the shape of the graves, there are some of an inverted cylinder form, like that of a lime-kiln, the insides of which are lined with masonry work. In these the body is placed in the upright position. There is also the ordinary longitudinal grave, in which the corpse is right in contact with the earth. Likewise the grave cut square to a depth of 6 to 8 ft., at the top of which, or within 1 or 2 ft. of the surface of the ground, is a roofing or covering of mat-work, placed on wooden rafters. In one of these Dr. Hutchinson, her Britannic Majesty's Consul at Callao, found three bodies, all wrapped up together—being a man, woman, and child—their faces being swathed with llama wool instead of cotton, as is usually seen in ordinary ones. He also turned out relics of fishing nets, with some needles for making them, varieties of cloth, tapestry, and work-bags, resembling ladies' reticules. Not a vestige of vegetation about, nor sign of relic of the terraces mentioned by Prescott. Whence came these hundreds and thousands of people, who are buried at Ancon? How did they make out a living while on the earth? Time and time again the archæologist finps himself face to face with such questions, to which he can only shrug his shoulders and say with the natives, "Quien sabe?" Who knows?

At Parmayo, 14 miles further "down north," and on the sea shore, is another great burying-ground. Thousands of skeletons lie about, thrown out by the treasure-seekers. It has more than a half-mile of cutting through it for the Ancon and Chancay R. R. It extends up the face of the hill from the sea shore to the height of about 800 ft., and being from a half to three-fourths of a mile in breadth, some idea may be formed of its extent.

Dr. Hutchinson, in two days, from these burial grounds gathered 384 skulls, which, with specimens of pottery, he presented to Professor Agassiz, and he to the Cambridge University, near Boston.

Between the teeth he found pieces of copper, as if for the Charon obolus, and one or two had plates of copper on their heads.

Crossing the brow of the hill, entering Chancay, and stretching towards the sea, are the remains of a 6 feet adobe wall. On the face of this hill, pointing to the line of the railway from Ancon, are two stone ditches, perfectly parallel and symmetrical, about 100 yards apart, and running from bottom to top to a height of about 300 yards. Between these are other lines of stones displaced, perhaps the ruins of some old terraces. All about this place, at the base of the hill, looking towards Chancay, as well as on the side in front of the sea, is full of graves; some are built up with stone walls, others lined inside with mud-bricks, of no formation more than a heap of clay and water moulded up in the hands and dried in the sun. Over the hills of Chancay are quantities of small stones of different geological formation from the rock there.

Dr. Hutchinson writes, under date of October 30, 1872, in an article to the Callao and Lima "Gazette," now the South Pacific "Times:" "I am come to the conclusion that Chancay is a great city of the dead, or has been an immense ossuary of Peru; for go where you will, on mountain top or level plain, or by the sea side, you meet at every turn skulls and bones of all descriptions."

Lima, the capital of Peru, is situated seven miles inland from Callao. Nine miles on the sea shore "up south," is the city of Chorillos, the Long Branch of Peru. A railway connects Lima with these two cities, forming with the coast nearly a right angled triangle. This triangular ground is known as the Huatica Valley, and is an extensive ruin. Between Callao and Magdalena, four miles distant, there are seventeen mounds called "huacas," although they present more the form of fortresses, residences, or castles, than burying ground. It is difficult to make out anything but fragments of walls, as the ground is mostly under cultivation. However, at various points, one can see that a triple wall surrounded the ancient city. These walls are respectively one yard, two yards, and three yards in thickness, being in some parts of their relics from fifteen to twenty feet high. To the east of these is the enormous mound called Huaca of Pando; and to the west, with the distance of about half

a mile intervening, are the great ruins of fortresses, which natives entitle Huaca of the Bell. La Campana, the huacas of Pando, consisting of a series of large and small mounds, and extending over a stretch of ground incalculable without being measured, form a colossal accumulation. The principal large ones are three in number; that holding the name of the "Bell" is calculated to be 108 to 110 feet in height. At the western side, looking towards Callao, there is a square plateau with an elevation of about 22 to 24 feet, 95 to 96 yards north and south, east and west. At the summit it is 276 to 278 yards long, and 95 to 96 across. On the top there are eight gradations of declivity, each from one to two yards lower than its neighbour; counting in direction lengthwise, the first plateau is 96 to 97 yards; second plateau, 96 to 26 yards; third plateau, 23 to 24 yards; fourth plateau, 11 to 12 yards; fifth plateau, 11 to 25 yards; sixth plateau, 23 to 24 yards; seventh plateau, 35 to 36 yards; eighth plateau, 35 to 37 yards; making a total of about 278 yards. For these measurements of the Huatica ruins I am indebted to the notes of J. B. Steere, Professor of Natural History and Curator of the Museum at Ann Arbor, Michigan.

The square plateau first mentioned, at the base, consists of two divisions, one 6 feet lower than the other, but each measuring a perfect square 47 to 48 yards; the two joining form the square of 96 yards. Beside this, and a little forward on the western side, is another square of 47 to 48 yards. On the top, returning again, we find the same symmetry of measurement in the multiples of twelve, nearly all the ruins in this valley being the same, which is a fact for the curious. Was it by accident or design? In its breadth from north to south three levels are found. The first lower down, 17 to 18 yards wide; the second or highest summit, 59 to 60 yards across; and the third descent again, 23 to 24 yards. Themound is a truncated pyramidal form, and is calculated to contain a mass of 14,641,820 cubic feet of material. For the most part, this great work is composed of adobes, each 6 inches long, 4 inches wide, and 2½ thick, many having the marks of fingers on them. But this does not consist of more than one-third of the Pando huaca.

Walking down past the southern corner, where the adobes are tumbled into a conglomerate mass by some earthquake, we see skulls with bones of arms and legs, cropping up in many places. The same adobe work is visible throughout, and the whole length of these structures range between 700 and 800 yards. The "Fortress" is a

huge structure, 80 feet high, 148 to 150 yards in measurement. Great large square rooms show their outlines on the top, but are filled with earth. Who brought this earth here, and with what object was the filling up accomplished? The work of obliterating all space in these rooms with loose earth must have been almost as great as the construction of the building itself. About two miles south of the last-named fort, and in a parallel line with it as regards the sea, we find another similar structure, probably a little more spacious and with a greater number of apartments, or divisions by walls, on the top of which we can now walk, as it is likewise filled up with clay. This is called "San Miguel." It is nearly 170 yards in length, and 168 in breadth, and 98 feet high. The whole of these ruins, big fortress, small forts, and temples were enclosed by high walls of adobones, but all of wedge-shaped form, with the sharp edge upward. Adobones are large mud bricks, some from 1 to 2 yards in thickness, length, and breadth. The huaca of the "Bell" contains about 20,220,840 cubic feet of material, while that of "San Miguel" has 25,650,800. These two buildings were constructed in the same style—having traces of terraces, parapets, and bastions, with a large number of rooms and squares—all now filled up with earth.

Near Lima, on the south, is another mound, 70 feet high and 153 yards square. Near the residence of Par Soldan, the Geographer of Peru, is a mound called "Sugar Loaf," or "San Isidro," 66 feet high, 80 yards broad at the base, and 130 yards long. Professor Raimondi, the naturalist, chemist, and scientist, who is doing for Peru what Gay did for Chili, said he found nothing in it but bodies of ordinary fishermen, relics of nets, and some inferior specimens o pottery.

Prof. Steere and Dr. Hutchinson turned out about forty skulls, some bits of red and yellow dyed thread, being relics of cloth; a piece of string made of woman's hair, plaited, about the size of what is generally used for a watch-guard; and pieces of very thick cotton cloth, bits of fish-nets, portions of slings, and two specimens of crockery-ware of excellent material.

About a mile beyond, in the direction of "Mira Flores," is Ocharan, the largest burial mound in the Huatica valley. This mound presents, as it is approached, the appearance of an imposing and enormous structure. It has 95 feet of elevation in its highest part, with an average width of 55 yards on the summit, and a total length of 428 yards, or

1284 feet, another multiple of twelve. It is enclosed by a double wall 816 yards in length by 700 across, thus enclosing 117 acres. Between Ocharan and the ocean are from 15 to 20 masses of ruins, like those already described.

Fifteen miles south of Lima, in the valley of Lurin, and near the sea, are the ruins of Pacha Camac, the Inca temple of the sun. Like the temple of Cholula on the plains of Mexico, it is a sort of made mountain or vast terraced pyramid of earth. It is between 200 and 300 feet high, and forms a semi-lunar shape that is beyond half a mile in extent. Its top measures about 10 acres square. Much of the walls are washed over with red paint, probably ochre, and are as fresh and bright as when centuries ago it was first put on. In these walls, in three or four places, are niches, apparently of the same shape and size as we see in the ruins of Pagan temples. From one side, going towards the north, are the relics of a wall, which is covered with soot, possibly the remnant of fires to make sacrifices, and nothing can better illustrate the conservative tendency of the Peruvian climate than the fresh appearance of the soot. Prescott says of Pacha Camac that it was to the Peruvians what Mecca is to the Mahometan, and Cholula was to the Mexican.

In the Canete valley, opposite the Chincha Guano Islands, are extensive ruins. In that region a terra-cotta mask was found, similar to that of which there is a drawing in Mr. Squiers's report of his explorations in the State of New York, and discovered while excavating for the St. Lawrence canal. From the hill called "Hill of Gold" copper and silver pins were taken like those used by ladies to pin their shawls; also, tweezers for pulling out the hair of the eyebrows, eyelids, and whiskers, as well as silver cups.

Buried 62 feet under the ground on the Chincha Islands, stone idols and water-pots were found, while 35 and 33 feet below the surface were wooden idols. Beneath the Guano on the Guanapi Islands, just south of Truxillo, and Macabi just north, mummies, birds, and birds's eggs, gold and silver ornaments were taken. On the Macabi the labourers found some large valuable golden vases, which they broke up and divided among themselves, even though offered weight for weight in gold coin, and thus have relics of greatest interest to the scientist been for ever lost. He who can determine the centuries necessary to deposit thirty and sixty feet of guano on these islands, remembering that since the conquest, three hundred years ago, no appreciable

increase in depth has been noted, can give you an idea of the antiquity of these relics.

The coast of Peru extends from Tumbez on the north to the river Loa on the south, a distance of 1233 miles. Scattered here and there over this whole extent, there are thousands of ruins besides those just mentioned, and similar, only not so extensive; while nearly every hill and spur of the mountains have upon them or about them some relic of the past; and in every ravine, from the coast to the central plateau, there are ruins of walls, fortresses, cities, burial vaults, and miles and miles of terraces and water courses. Across the plateau and down the eastern slope of the Andes to the home of the wild Indian, and into the unknown, impenetrable forest, still you find them. In 1861, Mendoza, in the Argentine Republic, a beautiful city on the plain, forty-five miles from the foot of the Andes, in the short space of five minutes was a complete ruin, and 15,000 out of her 20,000 inhabitants, or 75 per cent, were in the arms of death. In 1871 it was still exactly as on the evening of her destruction; the miles of skeletons lying uncovered where they perished, and the streets yet obstructed with the *débris* of the fallen walls of the houses. A new city has been built beside the old one. Seeking a photograph of the ruins, I was told there were none. Persuading one of the artists to take some views of them, and going to see the proof, he told me he had been out all day and had done nothing, as he could find nothing to take "but a pile of dirt." Thus, also, you might, as most do, style these coast ruins, and those who live among them understand and appreciate them no better than did the Mendoza artist the ruins of that ill-fated city.

In the mountains, however, where storms of rain and snow with terrific thunder and lightning are nearly constant a number of months each year, the ruins are different. Of granite, porphyritic, lime, and silicated sand-stone, these massive, colossal, cyclopean structures have resisted the disintegration of time, geological transformations, earthquakes, and the sacrilegious, destructive hand of the warrior and treasure-seeker. The masonry composing these walls, temples, houses, towers, fortresses, or sepulchres, is uncemented, held in place by the incline of the walls from the perpendicular, and adaptation of each stone to the place destined for it, the stones having from six to many sides, each dressed, and smoothed to fit another or others with such exactness that the blade of a small penknife cannot be inserted in any of the seams thus formed, whether in the

central parts entirely hidden, or on the internal or external surfaces. These stones, selected with no reference to uniformity in shape or size, vary from one-half cubic foot to 1500 cubic feet solid contents, and if, in the *many, many millions* of stones you could find *one* that would fit in the place of another, it would be purely accidental. In "Triumph street," in the city of Cuzco, in a part of the wall of the ancient house of the virgins of the sun, is a very large stone, known as "the stone of the twelve corners," since it joined with those that surround it, by twelve faces, each having a different angle. Besides these twelve faces it has its internal one, and no one knows how many it has on its back that is hidden in the masonry. In the wall in the centre of the Cuzco fortress there are stones 13 feet high, 15 feet long, and 8 feet thick, and all have been quarried miles away. Near this city there is an oblong smooth boulder 18 feet in its longer axis, and 12 in its lesser. On one side are large niches cut out, in which a man can stand, and by swaying his body cause the stone to rock. These niches apparently were made solely for this purpose. One of the most wonderful and extensive of these works in stone is that called Ollantay-Tambo, a ruin situated thirty miles north of Cuzco, in a narrow ravine on the bank of the river Urubamba. It consists of a fortress constructed on the top of a sloping, craggy eminence. Extending from it to the plain below is a stone stairway. At the top of the stairway are six large slabs, 12 feet high, 5 feet wide, and 3 feet thick, side by side, having between them and on top narrow strips of stone about 6 inches side, frames as it were to the slabs, and all being of dressed stone. At the bottom of the hill, part of which was made by hand, and at the foot of the stairs, a stone wall 10 feet wide and 12 feet high extends some distance into the plain. In it are many niches, all facing the south.

The ruins on the islands in Lake Titicaca, where Incal history begins, have often been described.

At Tiahuanaco, a few miles south of the lake, there are stones in the form of columns, partly dressed, placed in line at certain distances from each other, and having an elevation above the ground of from 18 to 20 feet. In this same line there is a monolithic doorway, now broken, 10 feet high by 13 wide. The space cut out for the door is 7 feet 4 inches high by 3 feet 2 inches wide. The whole face of the stone above the door is engraved. Another, similar, but smaller, lies on the ground beside it. These stones are of hard porphyry, and differ geologically from the surrounding

rock; hence we infer they must have been brought from elsewhere.

At "Chavin de Huanta," a town in the province of Huari, there are some ruins worthy of note. The entrance to them is by an alley-way 6 feet wide and 9 feet high, roofed over with sandstone partly dressed, of more than 12 feet in length. On each side there are rooms 12 feet wide, roofed by large pieces of sandstone 1½ feet thick and from 6 to 9 feet wide. The walls of the rooms are 6 feet thick, and have some loopholes in them, probably for ventilation. In the floor of this passage there is a very narrow entrance to a subterranean passage that passes beneath the river to the other side. From this many huacos, stone drinking-vessels, instruments of copper and silver, and a skeleton of an Indian sitting, were taken. The greater part of these ruins were situated over aqueducts. The bridge to these castles is made of three stones of dressed granite, 24 feet long, 2 feet wide by 1½ thick. Some of the granite stones are covered with hieroglyphics.

At Corralones, 24 miles from Arequipa, there are hieroglyphics engraved on masses of granite, which appear as if painted with chalk. There are figures of men, llamas, circles, parallelograms, letters, as an R and an O, and even remains of a system of astronomy.

At Huaytar, in the province of Castro Virreina, there is an edifice with the same engravings.

At Nazca, in the province of Ica, there are some wonderful ruins of aqueducts, 4 to 5 feet high and 3 feet wide, very straight, double-walled, of unfinished stone, flagged on top.

At Quelap, not far from Chochapayas, there have lately been examined some extensive works. A wall of dressed stone, 560 feet wide, 3660 long, and 150 feet high. The lower part is solid. Another wall above this has 600 feet length, 500 width, and the same elevation of 150 feet. There are niches over both walls, 3 feet long, 1½ wide and thick, containing the remains of those ancient inhabitants, some naked, others enveloped in shawls of cotton of distinct colours and well embroidered. Their legs were doubled so that the knees touched the chin, and the arms were wound about the legs. The wall has three uncovered doors, the right side of each being semicircular, and the left angular. From the base an inclined plane ascends almost insensibly the 150 feet of elevation, having about midway a species of sentry-box in stone. In the upper part there is an ingenious hiding-place of dressed stone, having upon it

a place for an outlook from which a great portion of the province can be seen. Following the entrances of the second and highest wall, there are other sepulchres like small ovens, 6 feet high and 24 in circumference: in their base are flags, upon which some cadavers reposed. On the north side there is, on the perpendicular rocky side of the mountain, a brick wall, having small windows 600 feet from the bottom. No reason for this, nor means of approach, can now be found. The skilful construction of utensils of gold and silver that were found here, the ingenuity and solidity of this gigantic work of dressed stone, make it also probably of pre-Incal date.

To support the inhabitants it became necessary to cultivate every part of the land possible; and since the greater portion is mountainous, they could make no use of that land except by such means as they adopted, *i.e.*, by terraces. Along the side, at the base of a hill or mountain, a stone wall is laid, from 1 to 8 feet high, according to the slope, and earth filled in between it and the side of the mountain, till even with the wall. Having this level for a base, another wall is laid, and again earth filled in, and so on, tier above tier, as high as the place will permit. These are terraces. The summits of the mountains are saturated with water from the melting snow or winter rains. This, forming little streams, is guided over these terraces. Each terrace is divided into patches by making a little ridge of earth a few inches high all around them, enclosing places 2 feet by 6, or 8 feet by 10, and so on according to the size of the terrace. The top terrace is first flooded, the ridge of earth serving as a dam. When it is considered wet enough, a channel is made by taking out a part of the ridge (with the hand, or a little paddle about the size of a pancake turner), permitting the water to escape to the part below, flowing over the wall to the next terrace, which is similarly treated. But there are thousands of terraces where the mountains and hills are so low and near the rainless portion that snow never, and rain very seldom, moistens their summits, and where no one could expect water for irrigation unless carried there by hand. Starvation alone would compel people to undertake so fatiguing and laborious a work, especially in a country where the evenness of the climate tends to relax the energy of both mind and body. Estimating five hundred ravines in the 1200 miles of Peru, and 10 miles of terraces of fifty tiers to each ravine, which would only be 5 miles of twenty-five tiers to each side, we have 250,000 miles of stone wall, averaging 3 to 4 feet high—enough to encircle this

globe ten times. Surprising as these estimates may seem, I am fully convinced that an actual measurement would more than double them, for these ravines vary from 30 to 100 miles in length, and 10 miles to each is a low estimate. While at San Mateo, a town in the valley of the River Rimac, 77 miles from the coast, where the mountains rise to a height of 1500 or 2000 feet above the river bed, I counted two hundred tiers, none of which were less than four and many more than six miles long. Even at four miles there would be at that point alone 800 miles of stone wall, and that only on one side of the ravine.

Who, then, were these people, cutting through 60 miles of granite, transplanting blocks of hard porphyry, of Baalbic dimensions, miles from the place where quarried, across valleys thousands of feet deep, over mountains, along plains, leaving no trace of how or where they carried them; people ignorant of the use of iron, with the feeble llama their only beast of burden; who, after having brought these stones together and dressed them, fitted them into walls with mosaic precision; terracing thousands of miles of mountain side; building hills of adobes and earth, and huge cities; leaving works in clay, stone, copper, silver, gold, and embroidery, many of which cannot be duplicated at the present age; people apparently vying with Dives in riches, Hercules in strength and energy, and the ant and bee in industry?

Callao was submerged in 1746, and entirely destroyed. Lima was ruined in 1678; in 1746 only twenty houses out of three thousand were left standing, and again injured in 1764, 1822, and 1828, while the ancient cities in the Huatica and Lurin valleys still remain in a comparatively good state of preservation. San Miguel de Piura, founded by Pizarro in 1531, was entirely destroyed in 1855, while the old ruins near by suffered little. Arequipa was thrown down in August, 1868, but the ruins near show no change.

Spanish writers refer all to Incal make, but Incal history only dates back to the eleventh century, and from that time to the Conquest is insufficient, nor do they speak of many of these works. It is granted that the Temple of the Sun, at Cuzco, was of Incal make, but that is the latest of the five styles of architecture visible in the Andes, each probably representing an age of human progress; therefore we are pretty certain that the imperial glories of the Incas were but the last gleam of civilisation that mounted up to thousands of years; that long before Manco Capac, the Andes had been the dwelling-place of races whose beginnings must have been coeval with the savages of Western

Europe. The gigantic architecture points to the Cyclopean family, the founders of the Temple of Babel and the Egyptian Pyramids. The Grecian scroll found in many places, borrowed from the Egyptians; the mode of burial and embalming their dead, points to Egypt as their similar, while the distaff, plough, manner of threshing and of making brick, are the same as when the Israelites were captives.

The hieroglyphics, to none of which as yet a key has been found, cannot be referred to the Incas, since they apparently had no knowledge of characters, but kept their records and accounts by means of a quippus, or knots and different coloured threads, as did those in Asia, China, Mexico, and Canada in ancient times, and they kept in each city an official whose business it was to keep and decipher their quippus. It was made of twisted wool, and consisted of a thread or thick string, from 1 to 18 ft. long, as a base upon which other threads or strings were attached. The different colours had different significations: the red, soldier or warrior; the yellow, gold; the white, silver or peace; the green, wheat or corn, and so on. In numerals, one knot signified ten; two simple knots, twenty: the knot doubly interlaced, one hundred; trebly interlaced, one thousand; two interlacings of this latter, two thousand. By setting apart a quippus for the military, another for the laws and decrees, another for historic events—*i.e.*, a separate quippus for distinct classes of ideas—the same knots could be used many times over, but to read them one must know to which class they belonged. Certain signs were affixed to the beginning of each "mother thread," as the base or principal string was called, by which the official could distinguish each. However, should an official visit another locality, these signs had to be explained verbally, also the signs representing local events, names of rivers, mountains, ships, cities, &c. Henee, a quippus was only intelligible, for the most part, in the place it was kept. Many quippus have been taken from the graves, in excellent state of preservation in colour and texture, but the lips that alone could pronounce the verbal key, have for ever ceased their function, and the relic seeker has failed to note the exact spot each was found, so that the records which could tell so much we want to know will remain sealed till all is revealed at the last day.

The skulls taken from the burial-grounds, according to craniologists, represent three distinct races.

The first, to which the name of "Chinchas" has been given, occupied the Western part of Peru from the Andes to

the Pacific, and from Tunebez, on the north, to the desert of Atacama an the south.

The second, called "Aymaras," dwelt in the elevated plains of Peru and Bolivia, on the southern shore of lake Titicaca, where they reside even to this day, being the only race that did not give up their language for the Inichua, or language of the Incas, when conquered by them.

The third, called "Huancas," occupied the plateau between the chains of Andes north of lake Titicaca, to the 9th degree of south latitude. The race were supposed to have caused the peculiar shape of their heads by mechanical means, as the Flat-head Indians with us, and the Conibos, a tribe that now live on the banks of the Ucayali, near Sarayacu, but the taking from a mummy of a fœtus of seven or eight months having the same configuration of skull has placed a doubt as to the certainty of this fact.

How changed! How fallen from their greatness must have been the Incas, when a little band of 160 men could penetrate, uninjured, to their mountain homes, murder their worshipped kings and thousands of their warriors, and carry away their riches, and that, too, in a country where a few men with stones could resist successfully an army! Who could recognise in the present Inichua and Aymara Indians their noble ancestry?

Their songs are typical of their condition, and are called "tristes," or sad songs. Always a duet in a minor key, and at night, as you hear it, it seems rather the expiring wail of some lost spirit than a human voice. It begins with a full inspiration of the lungs, and at the highest pitch of the voice, and ends with the expiration of the breath, in a low, long-drawn-out "andante pianissimo." The words are chanted, and often made up for the occasion. These are the words heard by a traveller from the lips of a young Indian mother, in the wild recesses of the Andes:

> "My mother begat me amid rain and mist,
> To weep like the rain and be drifted like the clouds.
> You are born in the cradle of sorrow,
> Says my mother; and she weeps as she wraps me around.
> If I wander the wide world over,
> I could not meet my equal in misery.
> Accursed be the day of my birth,
> Accursed be the night I was born,
> From this time, for ever and ever!"

Three times the Andes sank hundreds of feet beneath the ocean level, and again were slowly brought to their present height. A man's life would be too short to count even the centuries consumed in this operation. The coast of Peru has risen eighty feet since it felt the tread of Pizarro. Sup-

posing the Andes to have risen uniformly and without interruption, 70,000 years must have elapsed before they reached their present altitude.

Who knows, then, but that Jules Verne's fanciful idea regarding the lost continent Atlanta may be near the truth? Who can say that, where now is the Atlantic Ocean, formerly did not exist a continent, with its dense population, advanced in the arts and sciences, who, as they found their land sinking beneath the waters, retired, part east and part west, populating thus the two hemispheres? This would explain the similarity of their archæological structures and races, and their differences, modified by and adapted to the character of their respective climates and countries. Thus could the llama and camel differ, although of the same species; thus the algoraba and espino trees; thus the Iroquois Indians of North America and the most ancient Arabs call the constellation of the "Great Bear" by the same name; thus various nations, cut off from all intercourse or knowledge of each other, divide the Zodiac in twelve constellations, apply to them the same names, and the northern Hindoos apply the name Andes to their Himalayan mountains, as did the South Americans to their principal chain. Must we fall in the old rut, and suppose no other means of populating the Western Hemisphere except "by way of Behring's strait"? Must we still locate a geographical Eden in the East, and suppose a land equally adapted to man and as old geologically, must wait the aimless wanderings of the "lost tribe of Israel" to become populated?

Beside dead and speechless relics of the past, there exists a living antiquity. In 7° S. latitude, a couple of miles from the sea, there is a town of about 4000 inhabitants called Eten. They speak, besides the Spanish, a language that some of the recently brought over Chinese labourers understand, but differ in all other respects. They intermarry brothers and sisters, uncles and nieces, nephews and aunts, *i.e.*, promiscuously, with no apparent curse of consanguinity. They are exclusive, permitting no intermarriage into their number or with the outside world. They have laws and customs and dress of their own, and live by braiding hats, mats, and weaving cloths. They will give no account of when they came or from whence, nor does history mention them as existing before the Spaniards came, nor does it record their arrival since. Among them you will find no sick or deformed people, their custom being to send a committee to each sick or old person, and if they judge the

patient past recovery, or the aged past usefulness, the public executioner is sent and they are strangled. Eten orders it, they say, and none ever interfere with these orders.

Thirteen thousand years ago, *Vega*, or *α Lyræ*, was the north polar star. Since then how many changes has she seen in our planet! How many nations and races spring into life, rise to their zenith splendour, and then decay; and when we shall have been gone thirteen thousand years, and once more she resumes her post at the north, completing a "Platonic, or Great Year," think you that those who shall fill our places on the earth at that time will be more conversant with our history than we are of those that have passed? Verily might we exclaim in terms almost Psalmistic, Great God, Creator and Director of the Universe, what is man that Thou art mindful of him!

NOTICES OF BOOKS.

Bacon's Novum Organum. Edited, with Introduction, Notes, &c., by THOMAS FOWLER, M.A., Professor of Logic in the University of Oxford. Clarendon Press Series. Oxford. 1878.

THERE have been many editions of the "Novum Organum," annotated with greater or lesser care. The book is read at Oxford, and it appeals to the interests of a large section of the cultivated community of a nation, in its fourfold bearing upon the history of Philosophy, Logic, Literature, and Science. Prof. Fowler's edition is prefaced by a carefully-written Introduction, embodying the result of much literary research, and discussing all the principal points of interest connected with Bacon as a great thinker, and as a man who profoundly influenced the mode of thought of succeeding ages. This is followed by the text of the "Novum Organum," which is accompanied by numerous very exhaustive notes, in the preparation of the scientific portions of which the author acknowledges the assistance of Prof. H. G. S. Smith, Mr. Kitchen, and Prof. Clifton, while he gives frequent references to our most modern text-books, such as "Watts's Dictionary of Chemistry," Ganot's "Physics," Deschanel's "Physics," and Tyndall's "Heat and Mode of Motion."

Prof. Fowler commences his Introduction by giving the dates of the principal events in Bacon's life, and of the first publication of his writings. He then discusses the object of the "Novum Organum," and the nature of Bacon's philosophical opinions. An important section (pp. 22 to 43) relates to Bacon's scientific attainments and opinions. The author is compelled to admit the three charges most commonly brought against Bacon, but he finds something to plead in extenuation of each of them. "The first is that he was a *dilettante* in Science. The second, that he was imperfectly acquainted with the existing state of knowledge. The third, that he grossly exaggerated the defects of his own time, which, in spite of all that he says, was really one of great and fruitful intellectual activity." Even if we were to admit all this in its fullest extent, the services which Bacon rendered to Philosophy and Science would merit our warmest recognition. Prof. Fowler considers that the main peculiarities of Bacon's method and teaching may be embraced under four heads:—"(1.) The emphasis with which he insisted on the necessity of consulting and collecting facts, of going straight to Nature, of instituting observations and experiments before

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formulating general propositions. (2.) The gradual ascent from propositions or axioms of a lower to those of a higher degree of generality. (3.) The selection and comparison of instances in place of the old *Inductio per Enumerationem Simplicem*. (4.) The disregard of Authority, and the restraint of Fancy."

No one who has the least acquaintance with Bacon's works asserts that he was a great experimental philosopher: although Voltaire and Maclaurin have called him "the father of experimental philosophy," we know that Galileo was much more worthy of the title. Again, no one who is acquainted with the history of philosophy asserts that Bacon invented the Method of Induction. To mention no earlier researches, the discoveries of Galileo in Astronomy and Dynamics far exceeded any result obtained by Bacon in direct experimental or observational Science; while the method of induction had been employed long before Bacon's time by Leonardo da Vinci, Nizolius, and many others. But for all this Bacon did contribute most materially to the advance of Experimental Science, and to the development of the Logic of Induction. Moreover, he popularised Science, he insisted on the accumulation of experimental facts, he freed men's minds from the blind deference to the authority of Aristotle, and he advocated the employment of scientific investigation to the furtherance of man's estate and the increase of man's power over Nature.

We think that Prof. Fowler's estimate of the influence of Bacon on the progress of Science is a very just one. He has weighed each side of the question fairly, and in an impartial spirit, and he has discussed with judicial accuracy the merits or demerits of the most conflicting opinions. The notes which he has written in explanation of the scientific portions of the second book are in the main very precise and accurate. It is only here and there that we find obscurity or inaccuracy of diction, as, for instance in Note 91 (p. 563), "*Omnis enim vita, immo etiam omnis flamma et ignitio*," to which is appended the note—"Compression acts in this case by stopping the supply of oxygen which maintains the combustion"; or in Note 96 (p. 565), in which the theory of the transmutability of the elements is attributed to Bacon, while it was originated by the Greeks two thousand years before.

The Theory of Sound. By John William Strutt, Baron Rayleigh, M.A., F.R.S. Vol. ii. London: Macmillan and Co. 1878.

The second volume of this very valuable work consists of nine chapters, which treat respectively of Aërial Vibrations; Vibrations in Tubes; Special Problems connected with the Reflection

and Refraction of Plane Waves; General Equations; Application of General Equations; the Theory of Resonators; Application of Laplace's Functions; Spherical Sheets of Air, and Motion in Two Dimensions; Fluid Friction, and the Principle of Dynamical Similarity. The whole subject is treated with consummate ability, and the work affords a complete mathematical treatment of the subject.

The Moon, her Motions, Aspect, Scenery, and Physical Condition. By RICHARD A. PROCTOR. Second Edition. London: Longmans. 1878.

IN the second edition of this popular work the author has omitted a good many calculations, connected with the lunar theory, which were too difficult for the general reader. He proposes to reprint these in a separate volume on the "Geometry of the Lunar Theory," for the use of those who are acquainted with the higher mathematics. Otherwise this edition does not differ much from the preceding, except that the final chapter, on the moon's physical condition, has been enlarged. We may remind our readers that the author discusses in succession the size, mass, and distance of the moon; her motions and changes of aspect, and a complete study of the surface so far as it is known. In the chapter on Lunar Celestial Phenomena the lunar atmosphere, scenery, seasons, and eclipses are described. The author considers that the "moon was *shaped*, so to speak, when the solar system itself was young, when the sun may have given out a much greater degree of heat than at present, when Saturn and Jupiter were brilliant suns, when even our earth and her fellow minor planets within the zone of asteroids were probably in a sun-like condition." He inclines also to the belief that the process of contraction which the moon's surface underwent produced the processes of disturbance which have resulted in the present condition of the moon's surface. An admirable map of the moon, after Beer and Mädler, is given at the end of the volume, and some striking illustrations of the condition of the moon's surface are taken from Nasmyth and elsewhere. Altogether the work furnishes a very popularly written, and at the same time exact, account of the moon, and we are sure that it will continue to be largely read both by the scientific and the ordinarily well-educated reader.

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The Art of Scientific Discovery, or the General Conditions and Methods of Research in Physics and Chemistry. By GEORGE GORE, LL.D., F.R.S. London: Longmans. 1878.

ALTHOUGH we are of opinion that the faculty of original research must be inherent in a man, and cannot be often implanted in him,—in a word that *inuestigator nascitur non fit*,—we are very glad to welcome this admirable treatise from the pen of Mr. Gore. It is a sort of *Ultimum Organum* in the matter of scientific research. It is thoroughly Baconian, and may be regarded as a nineteenth century representative of the second book of the "Novum Organum." It is philosophical in tone, and is throughout characterised by the blending of great experience with logical deduction. It will be read with profit both by the student and by the most advanced investigator.

The work is divided into five parts, which are respectively devoted to (1) a general view and basis of scientific research; (2) general conditions of scientific research; (3) personal preparation for research; (4) actual working in original scientific research; (5) special methods of discovery. The first part treats of the distinction between the attainable and unattainable objects of research; the certainty of scientific knowledge; the general principles of science; and criteria. The author passes on to the consideration (Part 2) of the general basis of success in discovery. Herein he reviews the position of man as a discoverer in Nature, the general course and character of many notable discoveries, and the classification of scientific truths. The third part treats of personal preparation for research, in which the author points out not only the necessity of manipulative skill, but also that of imaginative power. The fourth part is devoted to the discussion of the actual working in original scientific research, and this will specially recommend itself to all scientific readers. The fifth and last division of the work treats of special methods of discovery.

We most cordially recommend this work to the attention of all lovers of natural truths.

Lessons in Thermodynamics. By ROBERT E. BAYNES, M.A., Lee's Reader in Physics. Oxford: at the Clarendon Press. 1878.

THIS work, which, although small, furnishes the only complete mathematical introduction in English to the Dynamical Theory of Heat, is based upon the subject-matter of a course of lectures delivered at the Clarendon Laboratory in Oxford, during the

Hilary Term of 1876. The work commences with definitions of the fundamental limits, and passes on to the methods of measuring heat. The third chapter treats of the conservation of energy, and in it the first law of Thermodynamics is fully discussed, together with Dr. Joule's experiments, and the general equation of equivalence. The fourth chapter is devoted to the transformation of heat, and includes the development of Carnot's function and the second law of Thermodynamics. Other chapters treat of change of state, the flow of fluids, and heat-engines. The kinetic theory of gases is omitted, because Mr. Watson's treatise on the subject contains all that is required.

Mr. Baynes has furnished frequent references to the most notable and most recent researches connected with his subject, and his treatise will become a standard work wherever the higher branches of Physics are taught from a mathematical point of view.

Hydrostatics and Pneumatics. By PHILIP MAGNUS. London: Longmans. 1878.

THIS forms the seventh volume of the "London Science Class-Book Series. It is divided into seven chapters, which discuss the principal facts connected with liquids and gases, at rest and in motion. The motion of liquids and gases is less completely studied than the statical problems in relation to these bodies; and the considerable results which follow, both in Nature and on the lecture table, from the diminution of pressure in the line of a moving fluid or gas, are completely ignored. The book, although intended for school purposes, is in the main somewhat too advanced for any but the older boys in a school: it will, however, be very useful for examination purposes and for special reading.

In a second edition we trust the author will introduce some mention of the experiments of his namesake, Prof. Magnus, on the motion of fluids, and will give one or two examples in each set of exercises of worked-out problems.

We cannot tell who is responsible for Fig. 35, p. 76, or whether the author has experimentally tried it; suffice it to say that if the flow of water through the pipe be at all rapid, the liquid, so far from being raised in the lateral tubes, will draw in air through them, and will afford no indication at all of the amount of pressure on the walls of the tube.

Electricity and Magnetism. By FLEEMING JENKIN, F.R.S., Professor of Engineering in the University of Edinburgh. Fourth Edition. London: Longmans. 1878.

THE fact that this work has already reached a fourth edition is sufficient evidence of its appreciation. It is based upon all the newest principles developed by Sir William Thomson, Prof. Clerk Maxwell, Mr. Latimer Clark, and the author himself, and it is quite indispensable for those who take up the study of Electric Telegraphy with a view to its application to practical purposes. The questions of quantity, intensity, resistance, and potential are fully discussed. Useful chapters are devoted to electrical and magnetic measurements, to the use of instruments, and to the whole subject of telegraphy.

We have no doubt that the work will continue to hold the high position which it has attained.

A Text-Book of Arithmetic for Use in Higher School Classes. By THOMAS MUIR, M.A., F.R.S.E., Mathematical Master in the High School of Glasgow. London: Daldy, Isbister, and Co. 1878.

A VERY comprehensive, clearly written book, with all the examples worked out at the end. We have no doubt it will be largely used in Scotch schools.

A Treatise on the Dynamics of a Particle. By P. G. TAIT, M.A., and W. J. STEELE, B.A. Fourth Edition. London: Macmillan. 1878.

THIS work leaves little to be desired, so far as the higher parts of the subject are concerned, either as to fulness of matter or excellence and variety of problems; but the elementary portion is hardly worthy of the rest. The chapter on Kinematics would be greatly improved if the analytical investigation in it were supplemented by a little Geometry. The discussion of the Laws of Motion is at times confusing to a beginner from the number of details presented to his notice, whilst the excessive brevity of treatment required by the plan of the work prevents sufficiently full explanation. The only fault in the book is that a beginner in Dynamics cannot read it.

Report of the New Jersey State Commission appointed to devise a Plan for the Encouragement of Manufacturers of Ornamental and Textile Fabrics. Trenton: Naar, Day, and Naar. 1878.

We have here, within the compass of one hundred pages, a storehouse of useful facts and abundant food for serious reflection. The State of New Jersey has issued a Commission to examine in what manner manufacturing industry may best be encouraged within its jurisdiction, and the Commissioners have evidently come to the only legimitate conclusion—that the prosperity of manufacturers can only be permanently secured by raising the standard of intelligence and education both among employers and employed. If we ask what kind of education is needed, we must at once perceive that none of the three grades of mental training, as supplied respectively to the lower, the middle, and the upper class in England,—that is, the rudimentary, the commercial, and the classical,—can answer the purpose. How fully the Commissioners agree with us appears from the fact that they consider the position of the United States "alarmingly unfavourable," whilst they recognise that the American "system of general primary education is more widely spread and more effective than in any country in the world; and while we have a larger number of schools, in proportion to population, than perhaps any other country, we are destitute of trade schools, and have extremely inadequate provision for industrial education of any kind and for any class of our people." If, then, the educational position of America, from an industrial point of view, is—in spite of its elaborate and carefully-worked "common-school" system—still to be regarded as alarmingly unfavourable, we must guard against expecting much from the recent steps taken in this country in the way of primary education. Setting on one side the palpable fact that all persons in England who really wished for elementary instruction could have acquired it even before the passing of the Education Act, we cannot see that either our "Board" or our "Denominational" schools will greatly increase the industrial or the inventive capabilities of our population. What we want is a system of training which shall fix the attention of the student upon things rather than upon words.

One point seems to us to require explanation. If, as the Commissioners expressly declare, "all Europe is a generation in advance of us" (America),—if in Germany "the visitor from the United States is oppressed by the apprehension that his own country, unprovided with such efficient and essential means of giving to the people a good industrial education, is likely to suffer severely,"—how is it, we ask, that America is so fruitful in inventions, many of them of undeniable value? Are we not

almost forced to conclude that an efficient patent-law goes very far to countervail the effects of a deficient system of secondary education? And, if this is the fact, should not our own patent-laws be amended in a totally different direction from that repeatedly proposed by the present Lord Chancellor?

The accounts given in this pamphlet of the various industrial schools—using the term in its natural sense—on the European continent are exceedingly significant. We find that the Zurich Polytechnicum consists of about a hundred professors and assistants, and numbers nearly a thousand students. It has an astronomical observatory, a large chemical laboratory, laboratories of research and special investigation, collections of models of engineering constructions, museums of natural history, architecture, &c., all extensive and rapidly growing. This important establishment is supported by a population of only three millions of people, and possessing probably less wealth than that of some single counties in England! Its revenues consist of £12,500, paid by the Swiss nation and the canton of Zurich, and not much more than £2000 from students' fees. If, therefore, the number of students is near a thousand, the average cost to each must be between £2 and £3 yearly! The Commissioners remark that "in this Institution a young man may acquire an education such as can be given him nowhere in this country (America)." Do we not now see some at least of the reasons why, in spite of great natural disadvantages, such as dear fuel and distance from the sea, Switzerland figures so honourably at the Paris Exhibition?

The manner in which the Commissioners sum up their remarks on the German system of education is profoundly significant:—"It is sufficiently evident that in Germany, even more than in France, the governing and the educated classes, instead of standing aloof from each other, and instead of forgetting—as is too generally the case in our own country—those great facts and those imperative duties which every statesman (?) does and every citizen should recognise, have worked together for the common good, and have given Germany a vantage-ground in the universal struggle for existence and wealth which is likely in the future to enable that country for many years steadily to gain upon all competitors. It is now generally admitted that Germany is the best educated nation of the civilised world: there is danger that the United States may, with reason, be reckoned the worst." We certainly should be the last to deny or doubt the value of the German educational system, which in many respects we consider a model for the world; but its industrial position is, nevertheless, not quite so assured. Did not an American paper, in summing up the results of the Philadelphia Exhibition, use the memorable words—"From all other nations we have learnt something; from Germany nothing"? Did not, on the same occasion, an eminent German professor preach to his countrymen a sermon

the text of which was "Billig und schlecht"? Do we not hear the Germans themselves lamenting over the industrial condition and prospects of their own country? It seems to us that in the present state of general commercial depression every country imagines the position of its rivals better than its own, their measures more judicious, and their future more assured. Is not the brilliant figure which Germany makes in the intellectual world the result of an impulse given half a century ago, but now gradually slackening amidst the hindrances of dominant militaryism and of a newly-awakened greed for wealth? From the expulsion of the first Napoleon the principle of Germany was "plain living and high thinking;" she preferred the man of genius, learning, and research, even if in a threadbare coat, to the mere accumulator of money, Is this still her creed, or is she not, in her haste to get rich, "killing the goose that laid the golden eggs"?

To return to our more immediate subject: the Commissioners are naturally forced to admit that Britain has no such system of secondary education as exists on the Continent. They quote the advice given by C. Graham Smith to all aspirants to the profession of civil engineering, to go to Paris and take the course of study prescribed at l'Ecole Centrale, or else to attend lectures at a German or Swiss Polytechnic School. Still they point out that much is now being done "to redeem Britain from the disgrace which at one time threatened her." They consider that it is in the industrial application of art-education "that the British nation has most wonderfully exhibited the energy and the thoroughness with which it usually carries on whatever work it may undertake in real earnest." Giving a brief sketch of the rise and progress of our schools of art, they declare that—"From a position far, very far, behind the continental nations, Great Britain has, in this one technical department, brought herself up into the very foremost place."

The thought here naturally suggests itself that what we have done in one department we may do also in others. We have merely to exert ourselves for industrial science as we have done for industrial art, and we shall soon be spared the humiliation of exporting raw materials and re-importing them after having undergone a transformation. We shall have, it is true, a struggle with the speech-makers, who justly fear that if we base our education upon Science their reign will be over.

We cannot better conclude this notice than by citing the following admirable passage:—"In the selection of instructors for the highest class of technical schools another qualification is demanded, so extremely rare that few universities even can boast the possession of more than a very small proportion of such men. The man who is needed in the school of technology, where advanced students and professional men are to be taught the highest studies applicable in their professional work, must

not only be a learned scholar and a good teacher, but he must be an investigator, capable of entering upon original researches and of solving practical problems. He must be able to 'add to the sum of human knowledge,' and to supply the many serious omissions and correct the many important errors with which text-books abound wherever they touch the practical side of their subjects." If the American people will put such men in the professorial chairs of their technical schools the industrial future of their country is secured.

The Annual Report of the Queensland Philosophical Society, 1877, with the President's Address. Brisbane: Beal.

THIS Report contains little to cheer those who take an interest in the progress of Science in general, and who wish in particular that the treasures of fact which Nature offers in such regions as Queensland should not continue to lie unobserved and unrecorded. The Council complain that "the number of papers read has been very small [only three!], and it is a matter for regret that so little interest is taken in the Society, as is evinced by members not bringing forward either interesting papers for discussion or any objects of interest which they may possess for exhibition." The names of the three members who have done their part for the credit of the Society are—Mr. G. Bennett, who contributes a paper on the "Mammalia of Australia;" Mr. Diggles, who produced a memoir on "Some New and Rare Specimens of Australian Birds," in which he describes four species new to Science; and Mr. Thorpe, who has urged the importance of collecting Meteorological Statistics.

As regards the Presidential Address, without in the least calling in question the ability displayed, we must point out that it is devoid of local character, and might have quite as appropriately been delivered in London, Paris, or Berlin. We submit that a general outline of the immense and varied harvest of phenomena which only awaits earnest reapers to be brought into the world's garner would have been more than the somewhat metaphysical disquisitions into which Sir James Cockle has thought proper to enter.

Preliminary Report of the Field-work of the U.S. Geological and Geographical Survey of the Territories for the Season of 1877. Washington: Government Printing-Office.

THIS issue gives merely a general sketch of the ground worked over, without entering into a detailed account of results. We

find it mentioned in brief that Mr. Scudder has made an inspection of the insectiferous shales of the Tertiary basin of Florissant, which he estimates to be at least fifty times as extensive as those of Œningen, in Southern Bavaria. From six to seven thousand specimens of fossil insects, besides some thousand plants, have been already received from this new locality, and as many more will be secured before the end of the season. It is not too much to hope that these treasures may throw some fresh light on the development of entomological forms, and on the successive appearance in point of time of the different insect orders and families.

In the botanical department, also, much may be expected. Two of the greatest authorities in this science—Sir Joseph D. Hooker, P.R.S., Director of the Kew Botanical Gardens, and Prof. Asa Gray—took an active part in the Survey, and made a careful investigation into the flora of the Rocky Mountains and the Sierra Nevada.

Ethnologists may also look forward with interest to the forthcoming account of a "forgotten people," who once occupied the country about the head waters of the San Juan.

Geological Survey of Victoria. Prodromus of the Palæontology of Victoria. Decade V. By Frederick McCoy. Melbourne: Ferres. London: Trübner and Co.

This work consists of figures and descriptions of organic remains found in the colony of Victoria. No systematic arrangement appears to be followed, animals of various classes and plants succeeding each other promiscuously. The illustrations are well drawn, and the descriptions are full.

The Journal of the Royal Historical and Archæological Association of Ireland. Fourth Series, Vol. iv., Nos. 33 and 34, January and April, 1878. Dublin: printed for the Association.

This issue contains a history of the kingdom of Ossory, with a list of its sovereigns. There is here nothing of scientific interest, if we except the suggestion that during the later geological periods the central portion of the county of Kilkenny was the basin of a large lake. The death of a king is mentioned as having taken place "A.M. 3817." We had hoped that chronolo-

gists would by this time have ceased dating from an event, or rather a process, of unknown and unknowable antiquity.

Memoirs of the Geological Survey of India. Palæontologia Indica; being Figures and Descriptions of the Organic Remains procured during the Progress of the Geological Survey of India. Indian Tertiary and Post-Tertiary Vertebrata (Vol. i., 3). Ser. X., 3.—Crania of Ruminants. By R. LYDEKKER, B.A. Calcutta: Geological Survey Office, London: Trübner and Co.

THE author here figures and describes all the Bovoid Ruminants in the Indian Museum, identifying as many of them as agree with those of Dr. Falconer's species of which figures and descriptions are extant, and discussing their affinities. He remarks that there are at present seven well-marked species of true Bovidæ in South-eastern Asia:—*Bubalus arni*, *Bibos gaurus*, *frontalis*, and *banting*, *Bos chinensis* and *indicus*, and *Poephagus grunniens*. Nowhere in Asia is there a true taurine ox. *Bubalus arni* is doubtless the direct descendant of the gigantic *Bubalus palæindicus* of the Nerbudda gravels and of the uppermost Siwalik beds. The fossil species now identified are *Bos namadicus*, *planifrons*, *acutifrons*, and *platyrhinus*, *Bubalus platyceros* and *palæindicus*, *Bison sivalensis*, *Peribos occipitalis*, *Hemibos triquetriceros*, and *Amphibos acuticornis*, all of which, except the first-mentioned, have been found in the Siwalik. *Bubalus palæindicus*—an animal of immense strength, far surpassing the living Arni—was doubtless contemporary with man, since stone implements have been found in the bone-beds from which its remains have been obtained. The Sivatheridæ—of which a new species, *Hydaspitherium megacephalum* (Lydekker), is here figured and described—are interesting as connecting the now isolated genus *Camelopardalis* with other families of the order,

The figures, which are beautifully executed, have been chiefly lithographed by a native artist, Rasick Lall Bose.

Ser. XI., 2.—Flora of the Jabalpur Group (Upper Gondwanas) in the Son-Narbada Region. By OTTOKAR FEISTMANTEL, M.D. Calcutta: Geological Survey Office. London: Trübner and Co.

THIS volume is devoted to Ferns, Cycads, and Conifers. In his summary the author points out that in the Jabalpur-Kach group there is an older flora with a younger flora; whilst in the Kota-Maleri group the flora is the younger element and the land

animals and fishes the older one. The flora of the upper Godwana series is of a similar type to that of the jurassic (lower and middle) floras of Asia and South-eastern Russia.

Ser. II., 3.—Jurassic (Liassic) Flora of the Rajmahal Group from Golapili (near Ellore), South Godavari District. By Ottokar Feistmantel, M.D., Palæontologist, Geological Survey of India. Calcutta: Geological Survey Office. London: Trübner and Co.

We have here a description of Fossil Ferns, Cycads, and Conifers found embedded in the brown sandstone of Golapili. The beds in which they occur are concluded to correspond in age with the Rajmahal group in the Rajmahal hills. The illustrations are beautifully executed, and we are glad to perceive that native skill is employed in the lithographic process.

Ser. IV., 2.—On some Remains of Ganoid Fishes from the Deccan. By Sir Philip de M. Grey Egerton, Bart., F.R.S., F.G.S.

On the Genus *Ceratodus*, with Special Reference to the Fossil Teeth found at Maledi, Central India. By L. C. Miall, Professor of Biology in the Yorkshire College of Science and Curator of the Leeds Museum.

On the Stratigraphy and Homotaxis of the Kota-Maledi Deposits. By W. T. Blanford, A.R.S.M., F.R.S., Geological Survey of India.

Calcutta: Geological Survey Office. London: Trübner and Co.

Sir P. Egerton's paper is a description of Ganoid Fishes belonging to the genera *Lepidotus*, *Tetragonolepis*,—a form hitherto only found in the lias of Bang and Boll, in Germany, and of Dumbleton, in Gloucestershire,—and *Dapedius*.

Species of fishes referred to the genus *Ceratodus* are widely and curiously distributed. They have been found in a blackish shale in Illinois (*C. Vinsolvii*, Cope); in the upper keuper sandstone of Worcester, at Aust Cliff, and in the oolite of Stonesfield (*C. lævissimus*, *obtusus*, *disauris*, and *Phillipsii*); in the triassic rocks of South Germany, where some eight species have been discovered; in India, at Maledi, whilst in the rivers of Queensland a species, *C. Forsteri*, still exists.

The age of the beds named in the title Kota-Maledi, but in the text repeatedly Kota-Maleri, seems a difficult problem:—"We have an association in the same group of beds of triassic reptiles and fish, liassic fish and plants, which may be either middle or upper jurassic, according as plants or marine animals are considered better evidence of age. If the latter are admitted

to be the most characteristic, the plants found in the Kota-Maleri beds would be upper jurassic in other parts of India."

Botany, Outlines of Morphology, and Physiology. By W. R. McNab, M.D., F.L.S. ("The London Science Class-Books," edited by C. Foster, F.R.S., and P. Magnus, B.Sc.) London: Longmans and Co.

This little work is, as its title expresses, a condensed exposition of the morphology and physiology of the vegetable world. The results of modern research are given briefly, but with considerable clearness. To those entering upon the study of Phytology it will prove a useful guide. The absence of an index strikes us, however, as a defect.

Invertebrata. By Prof. A. Macalister. ("The London Science Class-Books.") London: Longmans and Co.

The little work before us undertakes to furnish, in the brief compass of 136 pages, an account of all the invertebrate sections of the animal kingdom. From this short space a considerable portion must further be deducted for the three well-written introductory chapters on the nature of animals, their vital functions, on nomenclature and classification, mimicry, parasitism, distribution in time and space, &c. The difficulties of the task, therefore, have not been trifling, and we may congratulate the author on his success in omitting so little that is of primary importance, and in presenting so clear a view of his subject under disadvantages so great.

At the same time we cannot pronounce the work altogether unexceptionable. There are certain errors, typographical or clerical, which a little care in revision would certainly have obviated. Thus, on p. 131, we find *elytræ* instead of *elytra* as the plural of elytron. On the same page *Agriotes*—the coleopterous genus whose larvæ are so unfavourably known as wireworms—is misprinted *Agrites*. On p. 129 we read that "the larva of the common daddy-longlegs feeds on the roots of grass, &c." There are also errors of a more serious nature. On p. 115 is a figure showing the structure of an insect's body, and said to be a dragonfly. Dr. Macalister must be aware that it differs from a dragonfly in every part and organ, and agrees much more closely with a locust. On p. 131 the reader is told that "the destructive Colorado or potato-beetle somewhat resembles the ladybird, but is ten-striped, and not spotted." In a treatise

which, though brief and elementary, is scientific, it would have been better to point out that the potato-beetle, as belonging to the Chrysomelidæ, has four main joints in each tarsus, especially as the three-jointed tarsi of the ladybird had just been mentioned. On p. 129 we find the statement that "On the lower lip in the larvæ of some moths there is a tubular spinning gland, which, when the larva has reached its full size, secretes a silken cocoon within which it is enclosed." This unhappily expressed sentence will lead learners to believe that silk is secreted by some species only, instead of, as is the fact, by all lepidopterous larvæ. If grammatically construed it also conveys the meaning that the cocoon is secreted as such, instead of merely in the shape of a silken thread from which it may be spun.

In treating of the distribution of animals the author says:—"Sometimes the presence of one animal prevents the diffusion of others: thus in Africa the tzetze fly renders whole tracts uninhabitable by oxen and deer, which are destroyed by its poisonous bites." It so happens, however, that with the exception of the Mediterranean coasts deer are absent throughout Africa, in districts free from the tzetze as well as in those which it infests.

We hope to see these defects remedied in a future edition.

Vertebrata. By Prof. A. Macalister. ("The London Science Class-Books.") London: Longmans and Co.

In this little volume Prof. Macalister completes the brief view of the animal kingdom begun in his treatise on the Invertebrata. We are unable to agree with him in his estimate of the comparative importance of these two great divisions of the animal kingdom. In his introductory remarks he expresses the opinion that "they (the Vertebrata) present to us a greater number of interesting points in structure, function, and habits than all the other sub-kingdoms put together; and as they are, for the most part, of large size and of complex organisation, they require a more careful and detailed study than do the animals which make up the other sub-kingdoms." We grant, of course, the larger size and the superior complexity of the Vertebrata, but on all other points we are compelled to join issue. We consider that among the Invertebrata there is not only a much larger number of species, but greater range of structure, and especially of habits, than among the vertebrate animals. We are glad to find here the order Primates—freed, however, from the lemurs—in place of the Cuvierian groups of Quadrumana and Bimana, and to come upon no hints as to the propriety of referring man to a separate class, or even sub-kingdom. The seals are quite justifiably separated from the Carnivora, which order is thus restricted

to comprise the Felidæ, Canidæ, Viverridæ, Mustelidæ, and Ursidæ. But, observing this, we cannot help referring to a passage at the conclusion of the volume on the Invertebrata, in which the author observes—"The subdivisions of insects are by some looked upon as deserving of a higher than ordinal rank; but as the nature of a group depends on the nature of the range of structure in the forms comprehended therein, and not on the number of included individuals, we cannot but see that in each order of insects the component species are constructed so much on one type as to forbid us from making of them more than ordinal groups." Accepting the principle thus laid down for determining the value of groups, we submit that in the so-called order Coleoptera there are differences of type more important than such as are among Mammalia allowed to separate the order Pinnepedia from Carnivora.

In the remarks on the migration of birds we notice an oversight. It is stated that the "swallows, swifts, nightingales, and warblers visit this country about the middle of April, breed here, and then return to their winter quarters about the first week of October." This is decidedly incorrect as far as the swift is concerned, which leaves us in August.

In the chapter on reptiles the common viper is described as recognisable by its dark green colour. We have had very extensive opportunities for observing this species, which, thanks to the destruction of its enemy, the hedgehog, is now on the increase in Britain, and have always found the males of an ashy or silvery grey, and the females copper-coloured, whence the popular German name "Kupferschlange." Neither can we endorse the opinion that "the poison of snake-bites is rapidly fatal, death taking place within an hour in general." This is doubtless true as regards the cobra, but not of the bites of poisonous snakes in general.

The author's description of the structural characters, both of the Vertebrata in general and of the several classes, is perhaps all that could be expected in so brief a compass. We have great pleasure in noticing that in this and in its companion volume the copious index is made to serve, at the same time, as a glossary of technical terms—a most valuable feature.

A Treatise on the Pathology of the Urine, including a Complete Guide to its Analysis. By J. L. W. Thudichum, M.D. Second Edition. London: J. and A. Churchill. 1877.

Although this is the second edition of Dr. Thudichum's well-known treatise, yet the long interval which has elapsed since the publication of the first edition in 1858 and the present one, and the consequent necessity of recording the mass of work which

has been done during these twenty years, gives an importance to this new volume which is far greater than that usually *to be* attached to the re-issue of such a volume. Of this mass of work, here so ably recorded, a very large portion is due to the active and untiring personal investigations of the author himself. Few men have studied more deeply than Dr. Thudichum the chemistry of the urine in health and in disease, and his results in this field of investigation alone, apart from his masterly remarks on the chemistry of the bile and his still later discoveries in connection with the chemistry of the brain, claim and secure for him the foremost rank among contemporary workers in science.

What is especially valuable in the book, in addition to the mere record of facts, is what may be termed its historical aspect. To each substance of importance the author devotes a special chapter, and gives at the beginning of each a summary of its history, so that from the date of its discovery we are enabled to follow its development at the hands of the various chemists who have given it their attention. It may be said, in fact, that the author, not content with mere history, has repeated all experiments of any importance relating to his subject,—a method of procedure which enables him, in his critical remarks, to confirm or condemn with an authority not to be lightly gainsaid. As a special illustration of this the exhaustive chapters on Urea and Uric Acid may be cited.

A special chapter is devoted to Xanthine. This substance, first discovered by Marcet in 1819 in a calculus, was further discovered by Liebig and Wöhler, and shown by Strahl and Lieberkühn in 1848 to be a normal ingredient of urine. Ten years later (1858) Thudichum showed it to be a normal ingredient of the human liver. Its now extensively studied properties are graphically detailed, and its relation to hypoxanthine and guanine clearly pourtrayed. Its preparation by the phosphomolybdic process is of great value. By this process Thudichum obtains Kreatinine as such, and also two new bodies—Reducine and Urochrome. To these and similar bodies he applies the term "alkaloid" in a wider sense than is usually accepted. Reducine is a characteristic type of a remarkable class of bodies occurring also in the brain, and possesses, like these, in addition to its basic properties, co-existent acid properties, pointing to the probability of its being an amido-acid with marked basic tendencies.

The fact that hypoxanthine, a pathological ingredient of urine, occurs naturally in the brain of man and ox, though vaguely indicated by Müller, was first experimentally demonstrated by Thudichum. Guanine, although not occurring in urine, also receives special attention, on account of its chemical relations to the uric acid group.

Under Kreatinine, an undoubted ingredient of urine, we have

much interesting matter. It is important to note that Thudichum insists on the existence of kreatine in flesh and urine as by no means proved, since the processes of its extraction involve the transformation of kreatinine into kreatine. The question arises, in our mind, whether the phospho-molybdic process already mentioned—which is said to produce kreatinine from urine free from kreatine—would, when applied to the juice of flesh, yield a similar result.

The chapter on Hippuric Acid affords a strong interest, historical, critical, and chemical. The same remark applies to the next following chapter, on Indigogen and Urrhodinogen. The long and not unamusing controversy on these chromogen products—or, as their partisans insist on calling them, colouring-matters—is here reproduced. The theory that the yellow colour of the urine in certain diseases is due to an indigo-bearing substance is clearly stated, and still more clearly disproved by Thudichum. Thudichum demonstrates that the most colourless urine—viz., the first urine in the reaction from cholera—yields indigo-blue in greatest abundance, and that urrhodine (the red matter stated by many to be isomeric with indigo-blue) contains, unfortunately for this wild theory, no nitrogen whatever!

Passing over the chapters on Pyrocatechin, and the presence of phenol and cresol producing substances in the urine,—matters still surrounded with mystery,—we come to Schorling's Omicharyb-oxide, which Thudichum shows to be identical with the fallow-resin of Proust, discovered as far back as 1800. Next follows a brief description of Urerythrine, also first described by Proust as rosacic acid—a pathological ingredient of urine which imparts to it a fiery red colour, and is found among the "latentious deposits" along with the urates. Its spectrum has been specially examined and recorded by Thudichum.

We now come to Urochrome, "the matter to which urine owes the whole or the greater part of its yellow colour,"—an important discovery of Thudichum's in the year 1864, and for which he received the Hastings Prize Medal of the British Medical Association. It is impossible to enter into details relating to this body here; suffice it to say that the competent recognition it has received is fully justified by the mass of scientific data concerning it. The products of its decomposition—uromelamine, uropittine, omicholine, and omicholic acid—are also new to chemistry.

An equally important discovery of Dr. Thudichum's is kryptophanic acid, first observed by him in 1869. It is a normal constituent of urine, and the principal one of the bodies "which constitute the complexity of matters formerly called 'extractives.'" The detailed description of this remarkable substance, and the abundance of analytical proof concerning it and its sixteen studied salts, cannot fail to convince the reader of its chemical individuality. The occurrence of acetic and formic

acids in the urine has also received special attention at the author's hands.

With mention of these we conclude our notice of what for convenience may be termed the "organic" normal ingredients of the urine. Space precludes a similar notice of the "inorganic" matters. We must be content with mere enumeration of their divisions; to wit, carbonic acid, chlorine, chlorides of sodium and potassium, sulphuric acid and sulphates, hyposulphurous and sulphurous acids (a chapter full of new and original matter), hydrothion and hydrosulphocyanic acid, phosphoric acid and phosphates (also a most important chapter), ammonia, trimethylamin, potassium, sodium, calcium, magnesium, and iron.

Chapter XXXII., on the occurrence of the anatomical elements of the blood, and that following on Cruenturesis, will repay careful study, especially on account of their spectroscopical data. The chapters on albumen and grape sugar are also of first importance.

The rest of the volume is occupied with the consideration of specially morbid aspects, and of substances of slight, less frequent, or abnormal occurrence in the urine; and the book is fitly brought to a close by two chapters on the systematic qualitative analysis of urine, urinary sediments, and calculi. The average composition of normal urine given in the table on p. 563 is particularly useful for purposes of comparison.

The slight exposition here given of this work will be sufficient to show its importance both to the medical and chemical professions.

Etna; a History of the Mountain and its Eruptions. By G. F. Rodwell. London: C. Kegan Paul and Co.

We have here a monograph of what must be recognised as the greatest plague of Sicily, always excepting the Maffia and the police of "Italia redenta." The work is an expansion of an article prepared for the "Encyclopædia Britannica," but although accessible authorities have been consulted it must not be regarded as a mere compilation, since the author ascended the mountain during the summer of 1877, and obtained much information on the spot.

The first chapter is devoted to a notice of the principal writers, ancient and modern, who have given an account of the mountain and its eruptions, among whom Lucretius, Brydone, the Canon Recupero, Dr. Jos. Gemellaro, Elie de Beaumont, Baron Waltershausen, and Sir C. Lyell are the most prominent.

The physical features of this celebrated volcano are next described. The circle of vision from the summit has the enor-

mous radius of 150·7 miles, so that, to quote the author's illustration, were it placed at Derby a spectator on its highest point would be able to see the sea eastward of Yarmouth, the Isle of Wight southwards, the Irish Channel on the West, and the country beyond Carlisle to the north. A very remarkable feature is the dense population of the habitable zone of Etna, amounting to 1424 persons per square mile, a proportion almost equalling that of Lancashire, and due doubtless to the great fertility of the region. A characteristic of Etna is the great number of minor cones and craters, from all of which fire and lava have at times issued. Of these Waltershausen has mapped no fewer than two hundred within a 10-mile radius from the summit crater. Among the chief organic features of the region are the gigantic chestnut trees of Carpinetto, which are no myths, but sober realities. The trunk of one, according to the author, is 25 feet in diameter, whilst a public road passes through the much-decayed stem of the largest, the *Castagno di Cento Cavalli*. The author gives a brief account of the flora of the mountain, and dismisses the fauna with the brief and somewhat hazardous remark that "of course it is the same as that of the eastern sea-board of Sicily."

The author's ascent of the mountain is next described. He recommends travellers who undertake a similar expedition to select, as did he, the middle of summer, as the cold on the summit has been declared by some more severe than anything experienced in the Alps. If this opinion is based on sensations experienced, it may be due to the abrupt change from the semi-tropical temperature of the sea-coast.

Like Brydone, the author appears to have been much struck with the clearness of the sky and the brilliance of the stars. The spectroscopic lines he found defined with wonderful distinctness. Hence it is very satisfactory to learn that an observatory is to be erected at the Casa Inglesi, 1200 feet below the summit. Mr. Rodwell gives a very interesting account of the sunrise, and of the curious optical effects produced. The shadow of the mountain was projected across the island a hundred miles away, and seemed suspended in space at or beyond Palermo. The villages at the base of the mountain "seemed painted on a vertical wall in front of us." The chief hardship experienced is the change of temperature.

We have next a description of the principal localities in the Etnean region, including the ancient city of Adranum, whose temple was once "guarded by a thousand dogs." Aci Reale is noteworthy for its seven beds of suposed lavas, one of the earliest discovered pieces of evidence against the chronology of Usher.

After noticing the seventy-eight recorded eruptions of Etna, our author turns to the geology and mineralogy of the region. "The oldest eruptions must have taken place during the Glacial

epoch, but before the period of greatest cold in Northern Europe." It is noteworthy that, notwithstanding its vastly superior magnitude, Etna is far poorer in mineral species than is Vesuvius.

We must here conclude our brief survey of a book which cannot fail to prove attractive at once to the general reader, the historian, and the man of science, especially as there exists no other separate work on the same subject.

First Annual Report of the United States Entomological Commission, for the Year 1877, relating to the Rocky Mountain Locust. Washington: Government Printing-Office.

THE ravages of locusts have forced themselves upon the notice of mankind for some thousands of years; but in no country has this subject been dealt with in so thorough a manner as in the United States. Rightly judging that an accurate knowledge of the enemy must be the first step towards his destruction, an Act of Congress appointed a Commission to examine and report upon the "depredations of the Rocky Mountain locusts, and the best practicable methods of preventing their recurrence and guarding against their invasions." This Commission consisted, not as would have been the case in England, of lawyers, civil engineers, or sportsmen, but of three skilled and well-known entomologists—Messrs. Riley, Packard, and Thomas. The result has been the report now before us, which may safely be pronounced a mass of accurate evidence upon the ravages of the locust such as the world has not hitherto possessed. The national—and more than merely national—importance of the subject will best appear if we state that the direct and indirect losses sustained during the years 1874 to 1877 are estimated at not less than 200 million dollars, and that in certain districts immigration has been checked and the value of land depreciated. That the commissioners have been able to show how so gigantic an evil may be successfully combatted is strong testimony to the economical value of Entomology. It must not, however, be supposed that the report in question is merely utilitarian in its character. The authors have carefully investigated the distribution, the migrations, habits and vital conditions, the anatomy, embryology, and metamorphoses of the locust, and in studying its natural enemies—evertebrate as well as vertebrate—they have thrown useful side-lights upon a great number of animal species.

Much of the information here given will be no less useful in Australia, South Africa, &c., than in the United States, and the

Entomological Commission has therefore a just claim to public gratitude on both sides of the Atlantic.

Clavis Synoptica Hymenomycetum Europæorum. Conjunctis studiis scripserunt M. C. Cooke, A.L.S., et L. Quelet, M.D. London: Hardwicke and Bogue.

This work is a systematic account of the European species of plants belonging to the sub-order of the Hymenomycetes, one of the groups of Fungi to which the common mushroom belongs. The characteristics of each species are brief, rarely, if ever, exceeding two lines. The localities are not given, save that British species are marked with an asterisk in the margin. The great fault of the work is that it is written in Latin, a feature which might have been justifiable a century ago, when every man of scientific pursuits might safely be assumed to be a classical scholar, but which can now only be regarded as a survival of the unfittest.

The Freedom of the Truth. By Mungo Ponton, F.R.S.E. London: Longmans and Co.

From the title of this little work no very definite idea of its subject is likely to be gained. On reference to the Preface we find, however, that its aims are distinctly theological. The author attempts to show that "Christianity claims to be ranked as an experimental science, and that its principles ought to be studied by methods perfectly analogous to those followed in other experimental sciences. In this undertaking he makes a very frequent use of illustrations drawn from physics, chemistry, and biology, and in this latter department at least he scarcely shows himself as an unbiassed searcher for truth. There is the usual feeble attack upon the doctrine of Evolution, in which theological writers will still for some years think themselves bound to indulge. There is the somewhat stale anecdote of Lord Monboddo, and his belief that man was born with a rudimental tail, which "professors of the obstetric art" have to remove from newly-born infants. But Mr. Ponton does not seem to be aware that the real error of his "eccentric" countryman was his ignorance of the facts that the orang-outang and the gorilla are as free from outwardly visible tails as is man himself, whilst in all three species the internal rudiments of this dishonourable appendage are easily demonstrated. Our author, if he does not share this error, does not guard against being suspected of so doing, since

he simply writes that "man, being according to his views only a more perfectly developed variety of the ourang-outang, was probably born with a rudimental tail." Whilst pointing out the importance of keeping hypothesis in her proper place, he expresses his belief in "dominant ideas" whose "subtle and curious influence, united to the expectation of preconceived results, and the combined influence of these two on the brain and nerves," are to account for "all the remarkable phenomena of mesmerism, clairvoyance, and spiritualism." He does not, however, deem it necessary to enlarge on this topic, "seeing it has been so recently treated with great ability by Dr. Carpenter!" Mr. Ponton might perhaps have difficulty in finding a wilder hypothesis than that which he here adopts, nor does he rightly consider the terrible weapon which he is here putting into the hands of Materialists. For as the important "experiment" by which the Divine authority of the Scriptures is to be ascertained, he recommends prayer,—prayer in which we are to *expect* the fulfilment of our desires, our "affections and our emotions" being influenced. What reply, then, can he make if the Atheist turns his own arguments against him, and contends that the supposed answer to his supplications is merely the result of "dominant ideas" united to the "expectation of preconceived results," &c.? We do not, indeed, attach much importance to this argument, but then we are not accustomed to ignore facts, or what appear to be facts, without a careful and thorough scrutiny. To refuse such a scrutiny, even in cases where much error and delusion is probably mixed up with truth, we hold to be unworthy of Science.

Leisure Time Studies, chiefly Biological. A Series of Essays and Lectures. By ANDREW WILSON, Ph.D., &c. London: Chatto and Windus.

WE have here a collection of biological essays, pleasing, readable, and popular in the better sense of the word,—that is, intelligible outside of strictly professional and scientific circles, but not shallow nor inaccurate.

Perhaps the most interesting chapter is that devoted to "the Sea-serpents of Science." Taking for his motto the well-known lines "There are more things in heaven and earth, Horatio," &c., the author pleads for the reconsideration of a question which by some misfortune has been handed over to the jokers of the day to be dealt with at their indiscretion. In consequence of their exertion "*the* great sea-serpent—for no one ever condescends to speak of *a* sea-serpent—has been made the very type of mendacious exaggeration. It is even related that a

ship's captain, when told that some of his crew had sighted one of these mysterious monsters, absolutely refused to come on deck, since, to use his own words, 'Had I said I had seen the sea-serpent I should have been considered to be a warranted liar all my life after.'" It is surely much to be regretted that the elucidation of an interesting scientific question should be impeded or frustrated by childish ridicule. Following in the track of our late lamented Edward Newman, but with the advantage of having at his command much additional evidence, Dr. Wilson examines some of the principal alleged appearances of unknown sea-monsters, and points out the insufficiency of the explanations ordinarily given.

It is, in the first place, well known to zoologists—though perhaps not to the outside public—that veritable marine snakes inhabit the Indian and Chinese seas. These creatures are known to be very venomous, and have compressed fin-like tails adapted for swimming. More than forty distinct species have been captured and examined. Here, therefore, we have the proof that serpents inhabit the sea, and are met with at great distances from the coast. The only point, then, at issue is the size which a sea-serpent may attain. What length can we accept as possible, and what, on mere preconceived grounds, warrants us in pronouncing the narrator a liar? It must be remembered that marine animals are usually found larger than their nearest terrestrial allies. We know that boas and pythons have been found in South America and India not less than 30 feet in length. If, then, there are marine Ophidians bearing the same proportion to these that the whale does to the elephant, their size would not be smaller than that of the monster described by the crew of the *Pauline*. The only really valid piece of evidence against the existence of huge marine serpents is the fact that none of their bones have as yet been dredged up. Whether such a negative fact is sufficient to overpower the positive testimony of the various eye-witnesses may at least be doubted. Except there can be found in the narratives themselves something self-contradictory, we for one should at any rate lay aside our *a priori* notions, and vote for a suspension of judgment and the calm dispassionate collection of further evidence.

Dr. Wilson quotes from a weekly contemporary an explanation lately suggested. Dr. Jos. Drew saw a column of birds flying rapidly along the surface of the English Channel, and describes them as presenting the appearance of "an immense serpent, about a furlong in length, rushing furiously along at the rate of 15 or 20 miles an hour." Floating barrels, pieces of wreck to which sea-weed has become attached, and porpoises swimming in line are all said to have been, at first sight, mistaken for some huge oceanic serpent. Mr. Williams, in his interesting work "Through Norway with Ladies,"* suggests that low ridges of

* See Quarterly Journal of Science, vol. vii., p. 539.

rack stretching out into the Norwegian Seas may, under certain favourable conditions of the atmosphere, have seemed like the coils of a vast snake. We do not for a moment doubt but that all these, and perhaps other phenomena, may have occasionally passed muster for avatars of "the sea-serpent." But the question is, can all the described appearances be thus explained away? In three of the best-established cases quoted by Dr. Wilson—those of the *Dædalus*, the *Pauline*, and the *Georgina*—the problematical animal was seen either too close at hand or under such circumstances that all the above-mentioned hypotheses must be regarded as utterly inadmissible; and we have merely the alternative of branding the narrators as wilful and deliberate liars, or accepting their statements as substantially true. Now, great as may be the human tendency to falsehood, men of Teutonic or of Celtic blood may generally be depended upon when they have nothing to gain by a departure from the truth. What possible advantage could the crews of these three vessels hope to gain by the circulation of such narratives, and by what inducements were they brought to persist in the story in face of ridicule? We have heard, indeed, that the after life of Captain M'Quhæ, of H.M.S. *Dædalus*, was embittered, if not shortened, by the reflections cast upon his personal honour.

Our author, after describing certain other cases where the unknown animals seen were apparently other than Ophidian in structure, concludes "that many of the tales of sea-serpents are amply verified when judged by the ordinary rules of evidence: this conclusion being especially supported by the want of any *prima facie* reason for prevarication; that laying aside appearances which can be proved to be deceptive, and to be caused by inanimate objects or by unusual attitudes on the part of familiar animals, there remains a body of evidence only to be explained on the hypothesis that certain gigantic marine animals, at present unfamiliar or unknown to Science, do certainly exist, and that the existence of such animals is a fact perfectly consistent with scientific opinion and knowledge, and is most readily explained by recognising the fact of the occasional development of gigantic members of groups of marine animals already familiar to the naturalist."

A far stronger demand is made upon our powers of assent by the narratives concerning an underground monster of Southern Brazil—the "minhocao." Yet the eminent naturalist Fritz Müller, who is collecting evidence on this subject, sees reason for admitting that there is a substratum of truth in the exaggerated stories in circulation.

Will the time ever come when such books as the one before us will be to a great extent substituted for the literary rubbish, grave or light, which now fills the shelves of libraries and engrosses the leisure of the reading public?

Six Months in Ascension: or, an Unscientific Account of a Scientific Expedition. By Mrs. Gill. London: John Murray.

The Introduction to Mrs. Gill's "Unscientific Account" contains a short but most interesting history of the various attempts to measure our distance from the Sun, from that of Aristarchus of Samos, which gave about a twentieth part of the real value, to the plans of modern times, which are expected to come within one-tenth of a million of miles of the truth.

Mrs. Gill has said just enough to show some of the difficulties of this problem, and to increase the interest of her Expedition, without being technical and tedious; so that the most unscientific reader must read her Introduction with pleasure.

Mrs. Gill begins with a short description of the reasons and aims of her journey, and then goes on to their start at once, quite pathetically describing an accident that happened to Lord Lindsay's heliometer in Burlington House.

Ascension is an island in the Atlantic, about 800 miles north-west of St. Helena, and midway between the coasts of Africa and South America. It was discovered by Juan de Nova, on Ascension Day, 1501, whence it received its name.

No Government cared to take possession of 38 square miles of barren volcanic ash till 1815, when England, being afraid to leave a standpoint so near to St. Helena unoccupied, seized Ascension, and placed a captain and crew on board the *Flora tender*, as it is called in the "Naval Gazette."

The population consists of a company of marines, some St. Helena boys, and 70 or 80 Kroomen. The constitution is eminently monarchical, as the captain has the same power over his subjects as a captain of a man-of-war.

Mrs. Gill seems to have carried away the same impressions of Funchal as most other travellers; but as they were compelled to stay some time at St. Helena, we have an interesting description of that island, but for some uncouth Latin botanical names. *Polypodium*, for instance, is more like those dreadful things that frightened Hans Andersen's mermaid than a pretty fern.

Ascension gained, the first impressions of Garrison are not favourable. In spite of the Admiralty's gracious permission to buy meat, very little seems to be forthcoming, and that of a most peculiar quality. Hill, the servant, informs Mrs. Gill that as for sheep and bullocks "hardly any were killed that have not *fainted* first."

The first thing is to get the observatory up, of course; but no sooner is this done than envious clouds come in the way, and obscure the astronomer's view, till at last he decides to move his observatory, heliometer and all, across the "clinker," to another corner of the island,—a dangerous transit, for the clinker is rough, and the Kroomen insist on carrying the heliometer-case on their

heads. But all is accomplished safely, and a settlement is made at Mars Bay, as they call it, where the opposition of Mars is successfully observed.

But with the end of the work the end of the book by no means coincides. Excursions are made to the Green Mountain, Wide-awake Fair, and other places of interest, which Mrs. Gill describes as pleasantly and chattily as St. Helena; and only after Christmas spent in Ascension do they return home.

It is impossible to do justice to the lively, easy style in which "Six Months in Ascension" is written. From the opening chapter, with the Professor's story, to the return home at the end, interest never once flags, and there is no inclination to skip a word.

But why say any more? The subject is interesting, and if it were not it would be made so in the telling. The book must be read to be appreciated.

Section Cutting. A Practical Guide to the Preparation and Mounting of Sections for the Microscope. By Dr. SYLVESTER MARSH. With Illustrations. 87 pp. London: J. and A. Churchill.

THIS unpretending little book, professing to be scarcely more than a compilation made with a view in saving the student's time in searching numerous books and periodicals, is one of a class of treatises of which we have only too few.

The paste and scissors work of collecting and putting together scattered articles only requires the exercise of a little judgment and patience; but to try every process described, and in many cases to give modifications and improvements, as the author of this treatise has undoubtedly done, shows that the writing of only eighty-seven pages may involve a vast amount of real and original work.

Part I. of the book treats of Section Cutting in general, and describes all the well-known processes, with a great many useful hints, which will render the work much more hopeful to those who are commencing this difficult branch of microscopical investigation. The method of treating those tissues which require preparation before cutting is given very fully, considering the small space at the author's disposal. The Section Cutter, or Microtome,* is described at some length, the author laying great stress upon the necessity of obtaining a good instrument if fine sections are required. The points to be attended to in selecting

* This term in some microscopical works is applied to a cutting instrument made like a small pair of sheepshears.

a microtone are thus stated:—"(1.) That the cutting-plate of the instrument should be made of glass, or, in default of this, of very hard metal of the most perfect smoothness. (2.) That the diameter of the tube be neither too large nor too small; it ought not to be less than five-eighths of an inch or greater than 1 inch. (3.) That the screw, which should be fine and well cut, be provided with a graduated head. (4.) That there be some kind of index by which fractional portions of a revolution of the screw may be measured. And (5.) That the plug fit the tube of the microtome so accurately that when melted paraffin, gum, or any other imbedding agent, be poured into it, it may not find its way between the plug and the side of the tube." Attention is called to a very common fault—neglect of any means of preventing the rotation of the cylinder of imbedding substance, and keeping it from separating from the plug, a defect which entirely destroys the power of cutting sections of uniform thickness. The description of the process of cutting, and also the use of the freezing microtome, is given in so plain a manner that a mistake in carrying out the process is hardly possible. The author gives his experience of knives—a very important matter, an unsuitable tool being the cause of very many failures. The description of staining processes and the best mode of mounting the finished sections have not been neglected. A valuable hint is given respecting the exclusion of air-bubbles in "balsam mounting" during the application of the cover. It is recommended to dip the cover in turpentine before it is applied, when it will be found that "you can't get air-bubbles, even if you try."

Part II. is devoted to special methods, and in the paragraphs headed Bone, brain, cartilage, coffee-berry, fat, hair, horn, intestine, liver, lung, muscles, orange-peel, ovary, porcupine-quill, potato, rush, skin, spinal cord, sponge, stomach, tongue, vegetable-ivory, and wood, the student will find a collection of useful directions hardly to be obtained elsewhere.

A few blank pages for notes complete this valuable little book, which will prove as indispensable to the working microscopist as the very practical treatises of Carpenter, Beale, and Davies.

Victoria: Reports of the Mining Surveyors and Registrars. Quarter ending December 31st, 1877. Melbourne: J. Ferres, Government Printer.

THE total amount of gold obtained in the quarter, both from alluvial washings and quartz-reefs, is calculated at 218,159 ozs., and the exportation at 163,846 ozs. The grand total of mines employed in the gold-fields of the colony during the quarter is 38,055, so that the average quarterly yield of gold per man falls

short of 6 ozs.—a fact decidedly at issue with prevalent notions as to the lucrative character of gold-mining.

Flowers and their Unbidden Guests. By Dr. A. KERNER, Professor of Botany in the University of Innsbruck. With a Prefatory Letter by CHARLES DARWIN, F.R.S. The translation revised and edited by W. OGLE, M.D. London: C. Kegan Paul and Co.

THE part played by insects in the fertilisation of flowers since it was first distinctly pointed out by Mr. Darwin has given scope for a great amount of investigation, and has proved a delightful field of study both to botanists and entomologists. Indeed, it is difficult for any naturalist to enter upon this study without being compelled to notice simultaneously the structure of plants and the habits of insects, and thus to enlarge at once our knowledge of both. But whilst former observers have carefully ascertained the various contrivances by which self-fertilisation is prevented and by which insects are enticed to the flower precisely when their services are needed, Dr. Kerner has opened up quite a new phase of the mutual relations of plants and insects. All winged and creeping things are not alike welcome visitants to a flower. Those only of a certain weight, size, and shape, and, we may add, of certain habits, are likely to promote the great object of fecundation. Others would be useless or positively detrimental, and accordingly to prevent, as far as possible, their intrusion, we find a most interesting set of arrangements which the author here describes. He has thus succeeded in showing use and meaning in a vast number of floral structures which former observers had passed over as purposeless. By so doing he has invested the study of botany with new charms, and has encouraged men of science to persevere in their attempts to detect the function of parts not yet understood. It is interesting, however, to find that in this task Dr. Kerner has not been without a forerunner. Our lamented friend Mr. Belt, in his "Naturalist in Nicaragua," as Dr. Ogle points out in his preface, "distinctly recognises the fundamental point" of Kerner's memoir. "May flowers," he wrote, "have contrivances for preventing useless insects from obtaining access to the nectaries;" and again, "The structure of many (flowers) cannot, I believe, be understood, unless we take into consideration not only the beautiful adaptations for securing the services of the proper insect or bird, but also the contrivances for preventing insects that would not be useful from obtaining access to the nectar." This anticipation, however, is far from diminishing the interest and merit of the work before us. The idea which Belt first propounded Dr. Kerner has worked out in detail,

showing in a multitude of cases the various agencies by which "unbidden guests" are kept aloof. The author puts forth his work as a "small contribution" to the solution of one of the questions underlying the theory of natural selection. He complains that the phrase "the preservation of advantageous varieties" is often misused, and he recommends that instead of indulging in vague generalities on this preservation, we should solve the question at issue by the experimental method." No one can accuse him of neglecting to follow out his own advice. In his chapter on the advantages which accrue to the plant from bearing flowers, and especially from certain conformations of parts of the flower, we note that the word "biology" is used in a novel and, we fear, in a misleading sense. It generally signifies the entire science of living beings, but here we find it limited to "the determination of the functional significance of morphological characters. Dr. Kerner remarks very truly, "it is rare that any part of a plant is so shaped as to be suitable for the attainment of but one end," and he complains that naturalists have overshot the mark by trying to explain everything by the relation between the shape of the flower and that of the animals who visit it. In a special chapter he shows what insects and other animals are calculated to frustrate rather than subserve fertilisation, whether by destroying the flower, by consuming the nectar without rendering the desired services in return, or by preventing the visits of bees, moths, &c. Among such intruders a prominent part is played by ants, earwigs, and small beetles. Wingless insects are of little service, since even when they leave a flower laden with pollen, they cannot reach the flowers of a second stem of the same species until after a long journey and a proportionately long lapse of time.

The means of protection met with in different flowers against unbidden guests are explained in succession. Among these we find the secretion of distasteful substances, the hindrance of access by water, by viscid secretions, by prickles, hairy formations, by the diversion of visitors to other parts. The whole work forces upon our notice that constant strife which, whether we like it or not, must be admitted as pervading all nature. The flower seeks, as it were, to obtain the services of animals at the lowest market price. The animal, on the other hand, seeks to take the pay without doing the work, as when bees bite a side entrance into a flower so as to steal the nectar without effecting the desired exchange of pollen. Amidst such contrivances and counter-contrivances, every improvement on the one side being met by some fresh stratagem—if we may so term it—on the other, the notion of the "perfection" of every species, as inculcated by the old school of natural history, does not show to advantage. It reminds us of an old work in which the writer gave instructions for the "successful attack triumphant defence of any given fortress."

This charming work will, we are convinced, win new students for botany, and open out even to older observers fresh and fruitful paths. The editor has done his part well, and his preface and notes are a valuable addition to the volume. The work is illustrated with three double plates, well executed, and furnished with explanatory references. The general execution and appearance of the book are pleasing, and highly creditable to the publishers.

American Journal of Mathematics, Pure and Applied. Editor-in-Chief: J. J. SYLVESTER, LL.D., F.R.S.; Associate Editor-in-Charge: W. E. STORY, Ph.D., with the co-operation of D. PIERCE, F.R.S., in Mechanics, S. NEWCOMB, F.R.S., in Astronomy, and H. A. Rowland, in Physics. Vol. I., No. 1. Baltimore: the Editors; London: Trübner and Co.

THE appearance of this Journal is a proof of the growing attention paid in the United States even to those sciences which do not appeal directly to the sentiment of utility. For the character of the work the names of the editors will be a sufficient guarantee. The present number contains a "Note on a Class of Transformations which Surfaces may undergo in space of more than Three Dimensions," by S. Newcomb; in which the proposition is announced that if a fourth dimensiom were added to space, a closed material surface (or shell) could be turned inside out by simple flexure, without either stretching or tearing. Mr. G. W. Hill contributes, "Researches in the Lunar Theory;" Dr. H. T. Eddy a paper on the "Theorem of Three Moments;" G. Weicheld, the "Solution of the Irreducible Case;" H. A. Rowland, a "Note on the Theory of Electric Absorption," in which he concludes that electric absorption will almost certainly take place unless the ratio of conductivity to the specific inductive capacity is constant throughout the body.

Next follows a notice by G. S. Pierce of Annibale Ferrero's exposition of the method of least squares. From the pen of the Editor we have an "Application of the New Atomic Theory to the Graphical Representation of the Invariants and Covariants of Binary Quantics." This paper commences with the explanation that "by the *new* atomic theory I mean that sublime invention of Keklulé, which stands to the *old* in a somewhat similar relation as the astronomy of Kepler to Ptolemy's, or the System of Nature of Darwin to that of Linnæus. Like the latter, it lies outside of the immediate sphere of energetics, basing its laws on pure relations of form; and like the former, as

perfected by Newton, the laws admit of exact arithmetical definition." We think this paper will be read by chemists not without interest.

Geological Survey of Canada. Report of Progress for 1876-1877. published by Authority of Parliament.

THE work done in these two years comprises an exploration of British Columbia, by Mr. G. M. Dawson; a survey of the coal-fields of Nanaimo, Comox, Cowitchen, &c., by Mr. J. Richardson; a report on the Goderich Salt Region, by Dr. T. S. Hunt; report on geological researches north of Lake Huron, and east of Lake Superior, by Mr. R. Bell; surveys of the iron ores, apatite and plumbago deposits of Ottawa County, by Mr. H. G. Vernor; an account of the "Albert Shales" of Albert and Westmoreland counties, New Brunswick; notes on miscellaneous rocks and minerals, by Dr. B. J. Harrington; and an examination of Canadian graphites, by Mr. C. Hoffmann, from which they appear for the purposes of the crucible maker to be equal to the Ceylonese quality. Indeed the mineral wealth of the dominion is greater and more varied than is often supposed. The annual production of gold in British Columbia is considerable, but it is unfortunately not stated in ounces, but merely in dollars' worth. The argentiferous leads of Eureka, and of the Van Bremer Mine, show, acording to the assays of Dr. Harrington and Dr. Hunt, 271·48 and 347·08 ounces of silver per ton of 2000 lbs. Various well-authenticated specimens of cinnabar, native amalgam, and native mercury, have been met with; and Dr. Blake has found nickel among the heavy materials separated from the fine gold of the Fraser River. The exploration of British Columbia is as yet, however, exceedingly imperfect.

Dr. Hunt finds the salt from the Goderich deposits remarkably pure, and calculates that every twenty acres of this layer, if mined out, would be sufficient to supply the whole yearly consumption of the United States.

The principal palæontological facts recorded relate to the fossil insects discovered by Mr. G. M. Dawson, at Quesnel, in British Columbia, and examined by Mr. S. H. Scudder.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 2. Washington: Government Printing-Office.

This issue contains "Field Notes on Birds observed in Dakota and Montana along the 49th parallel, during the Seasons of 1873 and 1674," by Dr. Elliott Coues. The most interesting result is the capture of a newly-fledged Bohemian waxwing, showing that this species must breed at no great distance.

The next paper consists of "Notes on a Collection of Fishes from the Rio Grande, in Texas," by Dr. D. S. Jordan. Mr. A. R. Grote follows with "Preliminary Studies on the North-American Pyralidæ," Dr. C. A. White with "Descriptions of New Species of Invertebrate Fossils from the Laramie Group"—strata extending through Colorado, Wyoming, and Utah. The species collected, with the exception of a few insects and crustaceans, are all molluscous.

Dr. Hoffmann gives an interesting account of the mineral species found in Nevada.

The Scottish Naturalist: a Quarterly Magazine of Natural History. Edited by F. Buchanan White, M.D., F.L.S. No. 30. April, 1878. Edinburgh: Blackwood and Sons.

Is the increasing number of journals devoted to Natural History the "outward visible sign" of a corresponding increase in the amount of facts recorded and of work done? Or is it merely a distribution over a wider field of the subject matter formerly found concentrated in a few organs? If the former, as we trust, is the true supposition we wish these new comers every success.

The issue before us contains the continuation of an essay on Migration, by Col. Drummond Hay. It contains a number of interesting facts, but the author seems to take exception to Mr. Wallace's remarks on the dangers incurred by migratory birds in crossing the ocean upon mere *a priori* and teleological views. He considers that in "Nature's proper course there shall be no undue loss of life, and that everything should fulfil its own special purpose and maintain its own proper balance; and though man may disturb it by the ruthless destruction of many creatures, that in no way sets aside the design of a beneficent Creator."

It is difficult to comprehend the relevancy of such a passage, unless the author is seeking to contend that species never become scarcer or disappear without human intervention.

The papers on the "Lepidoptera of Moncreiffe Hill" and on "Glen-Tilt, its Fauna and Flora" are contributions towards the task of mapping out the range of every animal and vegetable species within this island—an operation which, as we learn from too many sources, is fast being simplified by the extirpation of many interesting animals and plants.

A significant paragraph by Mr. J. H. Gurney, of Norwich, points to the increase of wood-pigeons in Scotland, and the injury which they inflict upon crops, as facts not unconnected with the slaughter of 2642 individuals of the hawk tribe on three estates only. This is a fact which the author of "Game-Preservers and Bird-Preservers would do well to take into serious consideration.

The paper by Dr. Lauder Lindsay on the "Gold-fields and Gold-diggings of Crawford-Lindsay" has, perhaps, more of an antiquarian than of a mineralogical interest.

OBITUARY.

THOMAS BELT, F.G.S,

IN the obituary of the year, and amongst the list of scientific men who have loved science for itself, and sought truth for truth's sake, few will leave a brighter or happier memory with their friends than Thomas Belt.

Born in Newcastle in 1832, he was an early member of the Tyneside Naturalists' Club, and there began that love of nature and nature's ways that ever remained fresh with him throughout his life.

In 1852 his adventurous nature took him to the Australian Gold Diggings, and there (the leading spirit of a family of four brothers located in the colony), from 1853 to 1860 he successively visited, as a miner, the districts of Friars and Forest Creeks, Maryborough, Mount Molingul, Kingower, Korong, Mount Egerton.

In this rough "School of Mines" he acquired that practical knowledge which not only served him so well in after life in his profession, but gave him that insight into the building-up of the earth's crust which enabled him, not seldom, to put forth novel theories in geology and natural phenomena. Unorthodox as they were when first promulgated, yet, silently and solidly, they commended themselves to those who studed the facts and the inferences he drew from them. Amid real hard work in Australia, he found time to speculate on the flight of birds, and to show that the mechanical action of the bird's wings is not always the prime mover, but that the force of the wind, particularly in the Albatross, is the real agent that carries them sweeping over the ocean with the rapidity of the wind itself. Further, that this force is utilised by the faculty the bird has of balancing itself against the power of the wind. It is the equivalent of the string of the boy's kite, and almost overwhelming proof of this theory is afforded by the fact that the albatross is helpless in a calm, and cannot—from a level surface, as the deck of a ship—raise itself or fly so well as a domestic goose.

His theory of whirlwinds, viz., that the upper strata of air, pressed upon the lower rarefied and lighter strata, till a casual opening or thinning out in the upper layer leaves the lower strata free to fly upwards, and to form the circular whirlwinds common even in this country, was an outcome of his actual experience acquired in the dreadful dust storms of Australia. It is a curious fact that the paper on this subject, sent to a Melbourne scientific society, and put aside as unworthy of notice, was sent by Mr. Belt to the present Astronomer Royal, and then, as communicated by the Astronomer Royal to the Philosophical Institute of Victoria, was accepted and read in Melbourne, in December, 1857. The paper itself will be found in the "London, Edinburgh, and Dublin Philosophical Magazine" for January, 1859.

The boldest of his speculations, and one of the soundest, as after events proved, was his plan for crossing the Australian continent. He proposed, at the time the Government Expedition was mooted to replace the costly plans of the Government by the following scheme:—That he and his brother Anthony (who was unfortunately lost in the *Royal Charter*), should be conveyed to the Gulf of Carpentaria, with about twenty pack-horses loaded with provisions and water; that an escort should protect them for some twenty miles from the coast, and that then the two voyagers only, with their pack-horses should make their way to Cooper's Creek, the farthest known accessibl point from the Victorian settled districts. Belt argued justly: "If we fail, only two lives will be lost, but all the chances are in our favour; we are provided with water and food more than ample to cover the distance we have to travel. Every step of our road carries us homewards and to safety. If we

never find a drop of water on the road, our animals have enough to carry those who have to bear the whole journey to their goal, and as the animals succumb they will be shot or turned adrift." The event showed Belt's sagacity. The unfortunate Government Expedition left Melbourne loaded with camp followers and *impedimenta*, and by the time they reached a few stages beyond Cooper's Creek were well-nigh exhausted. Burke, the leader of the expedition, in desperation started with his two men, Wills and King, and bravely struck out for the Gulf of Carpentaria. Through desert and fertile plains, not altogether destitute of water, they reached in safety the northern shore of Australia; but the energy, the courage, and the strength that took them this long, weary journey did not suffice to carry them back over double the distance to their camp. Brave hearts! they struggled on, but King only, and as a worn out man, ever saw Cooper's Creek again. Belt's plan would have solved the problem without loss of life, and at a tenth of the cost. His ideas were in advance of his time, and he had that belief in his own powers which should have won his plan the attention its merits deserved. The writer knows the fact, that had Belt then possessed the means, he would have spent them all in his endeavour to carry out this scheme of crossing the Australian continent.

In 1862 Belt returned to England, and his professional engagements led him to North Wales, Nova Scotia, Central America, and Chontales Nicaragua. At the latter place his entomological collection has made him famous. Many hundred species of coleoptera and lepidoptera attest his energy and labour; and his charming book, "The Naturalist in Nicaragua," whilst illustrating his great powers of observation, has endeared him to every lover of nature, and proved the painstaking truth with which he collected his facts.

The succeeding years of his life were spent in almost continued travel: to North and South Russia, Siberia, the Kirghese Steppes, and many times to the United States. In these journeys he, from time to time, made those observations upon glacial action, upon which he built up his theories of the Ice Age. These became the ruling passion of his later years. Much of this work will be found in the "Quarterly Journal of Science" and in the "Quarterly Journal of the Geological Society." How much of it will stand the test of time the future only can tell; but all this special work of his is, at least, a careful and elaborate argument, advocating the theory that the extraordinary changes of climate in past ages, over large areas of the earth's surface which are now temperate regions, during the period called by geologists the glacial epoch, may have been brought about by other causes of less intensity than the submergences and emergences of the land, even than by the displacement of whole continents, which theories have been advanced by some to account for the phenomena in question.

Mr. Belt advocates the agency of ice, and ice dams, and great lakes—to use his own words—in place of "great upheavals and depressions of the earth's surface within a comparatively short period," and he questions the hypothesis by which "we are taught that an immense area in Europe and America has been a sea bottom, and every part of it a sea beach as the land rose again, without any evidence of marine life having been left behind;" and he claims that his theory of glacial action "explains all the phenomena by one great advance southwards of the ice of a single glacial period."—*Quarterly Journal of Science*, "Loess of the Rhine and the Danube," January, 1877.

The immediate cause of his death was brain fever, following a long attack of mountain fever. He died at Kansas City, United States, on the 22nd day of September, 1878, in his 46th year.